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"DAPHNE" DISASTER.

R E P O R T

ON THE

"DAPHNE" DISASTER

BY

SIR EDWARD J. REED, K.C.B., F.R.S., M.P.

Presented to both Houses of Parliament by Command of Her Majesty.

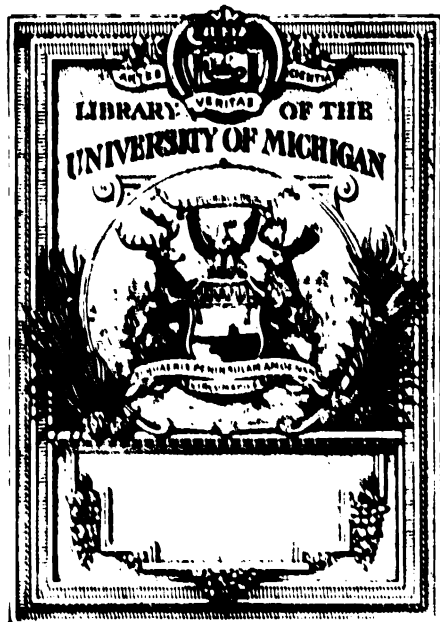


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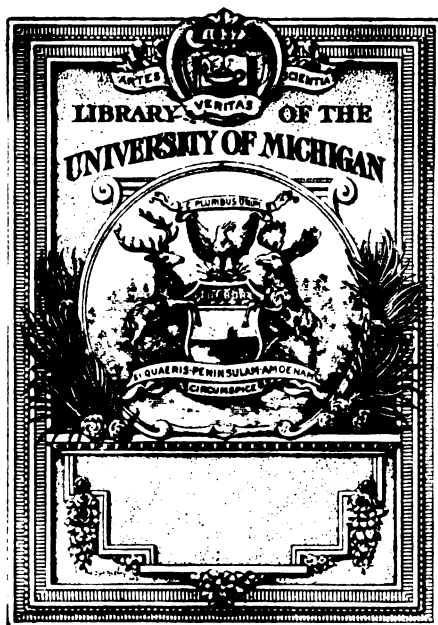
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[C.—3764.] Price 3s. 8d.



R E P O R T
ON THE
“DAPHNE” DISASTER.

THE RIGHT HON. SIR WILLIAM VERNON HARCOURT, M.P.,
SECRETARY OF STATE FOR THE HOME DEPARTMENT.

Broadway Chambers,
Westminster, London, S.W.,
August 14, 1883.

SIR,

IN accordance with your commission of the 7th July, conveyed to me through the Lord Advocate of Scotland, I have twice visited Glasgow and inquired into the cause of the capsizing of the “Daphne” with that care and deliberation which so serious a disaster has demanded. By the Lord Advocate’s desire the sheriff of Lanarkshire, and also the Procurator Fiscal, facilitated my inquiry by all the means within their power, and greatly contributed to its early completion. It is right that I should also state without delay that Messrs. Alexander Stephen and Sons, the builders of the “Daphne,” have tendered the fullest information respecting all the facts and circumstances of the case, and most readily complied with every request and suggestion that I have made.

For reasons which will presently appear, I find myself obliged to introduce into this report certain scientific and technical considerations of great importance which have been unexpectedly brought into view by this disaster, and have a decisive bearing upon it; and I therefore propose to divide what follows into two parts, in the first of which I will set forth the facts of the case in so far as I find them susceptible of treatment in general terms, while in the second part will be discussed those aspects of the case which necessarily involve a more technical treatment.

PART I.

The inquiry being limited to the cause of the disaster, I have not been under the necessity of doing more than thoroughly satisfy myself on that point; but there were, not unnaturally, many and various causes suggested, after the accident, in letters to the Press and otherwise, and I have been very careful not to pass over without due consideration anything that was in the least degree likely to throw light upon the matter. I may say at once, however, that very early in the inquiry it became quite evident that the cause of the accident was not to be found outside of the “Daphne” herself, in so far, at least, as the launching appliances were concerned. Many accidents have occurred to ships from defective launching arrangements, but all the evidence on this point goes to show that this ship was well and effectually launched; and a personal inspection of the groundways, bilgeways, and other appurtenances of the launch convinced me that they were all in good order, and sufficient, not only for the launching of the “Daphne,” but also for the successful launching of a ship more than double her size and weight. I may also state without delay that the accident is not traceable to the means adopted for checking the velocity of the vessel after she was afloat. Some means of this kind are indispensable in a narrow river like the Clyde, and are generally employed in such cases there and elsewhere; and although the means frequently adopted, and adopted in this case, viz., chain cables with iron plates, masses of chain, and half-buried anchors attached to them, are of a somewhat rude description, and will doubtless give place at some future time to better arrangements, they nevertheless answer the purpose desired, and answered it in this case, I am satisfied, without contributing to the disaster. The check-chains were attached to the ship differently on the port side and on the starboard side, the port chain passing through the hawse-pipe close to the stem of the ship, while the starboard chain was

attached to the side of the ship, in order, doubtless, to act as a check to the tendency of the tide to carry the stern up the river. Had these check-chains been disposed in the reverse way, with the port chain attached to the side, their action might have been supposed in some degree to have assisted in inclining the ship; but arranged as they were, their action on the whole was probably to resist the inclination. It was suggested in evidence by M. Benjamin Normand, a member of the eminent French shipbuilding firm of Normand, of Havre, that the real cause of the accident was presumably to be found in the fact that, however successfully the ship may have been launched, and whatever may have been the stability with which she afterwards floated, "there is a transition point" between her position upon the sliding ways and her position afloat "at which the vessel is no longer stable and safe." This view does not possess the novelty which M. Normand supposed; it has always been well understood that at the moment of a launching ship abandoning her ground supports, and before she has, by "dipping" forward, reached her floating trim, she passes through a phase of reduced stability, often doubtless of instability. But the duration of this state of things is so brief that it can hardly have any greater effect than that of setting up some little inclination, which probably accounts in part for the occasional slight rolling of a ship after launching. The worse case of a vessel having her fore foot striking or being supported upon the ground while the stern is afloat (which I have seen happen more than once at a launch) was fully investigated, for a somewhat different purpose, by Mr. F. K. Barnes, of the Admiralty, Whitehall, many years ago.* No doubt the passing phase of instability which the just-launched ship may momentarily experience is deserving of consideration, especially when other circumstances co-operate with it; but where the ship was well launched, as the "Daphne" was, and takes the water with a proper velocity, as she did, its effect is, in my judgment, too small and too transient to furnish the necessary force for capsizing the ship. I am confirmed in this view by the obvious consideration that a very large proportion of the ships that are launched pass through this brief and partial loss of stability with perfect safety. Moreover, there are in operation, tending towards danger, causes of greater magnitude, and of longer duration, than that upon which M. Normand laid stress.

At this stage of the inquiry a new suggestion enters into view; it is to the effect that the capsizing of the "Daphne" was due to a tidal cause. Mr. Robert McMaster, the manager of Messrs. Stephen's works, was the first to express this in formal evidence by saying, "My opinion is that the ship was entirely tripped by the flood tide coming up and the current coming down." Mr. William Pearce (J. Elder and Co., Glasgow), a shipbuilder of scientific training, and of very large experience in the launching of ships on the Clyde, so far corroborated this view as to say:—"I may say from my experience in launching ships, which has extended over a great many years on the Clyde, I have observed that during the last half hour of the flood tide there has often been a considerable rush up of surface water. I know it to be surface water from this fact, that although the current is apparently greater at that time than at any other time of the flood, the rise of water does not come up in the same proportion as it would do in any other time of the flood. From that reason I have always been of opinion that there is a down current below this surface water, and when I attended the scene of the accident at Messrs. Stephen's yard about 20 minutes or half an hour after it occurred, my first observation was directed to that. I thought the cause of the accident might be attributed to that." Mr. Robert Mansell, another Clyde shipbuilder, and a man of scientific skill, expressed substantially the same belief in the existence of contrary currents. Mr. James Deas, C.E., the engineer of the Clyde Trust, who has conducted tidal experiments on the river, was unable, however, to adduce any facts in corroboration of this view. He regarded the Clyde as a comparatively sluggish stream, with no appreciable differences of velocity at different depths. Some of the experiments made under his superintendence bore upon this particular point, and afforded no apparent indication that such differences of velocity existed. It appears to me unnecessary to say more on the matter than that, whatever reverse tidal currents may exist in the Clyde, it has certainly not been shown to my satisfaction that the "Daphne" was capsized by river currents, or that we can look to this cause for anything more than a possible small contribution to the inclining forces which were at work upon the ship.

We may confidently assume that the "Daphne" safely entered the river, and had the speed acquired during the launch taken out of her by the combined action of the resistance of the water and the drag of the check-chains. She then, according to all the

* See a paper in No. 4, *Annual of the Royal School of Naval Architecture*.

evidence, began to incline to port. Witnesses differ somewhat as to the mode of the capsizing which followed, some alleging that, after first rolling to port, she made a decided roll back towards the upright position; while of two competent observers who happened to be looking at the launch along the line of the vessel's length from the opposite side of the river, one states that the movement to port seemed to him to continue without intermission, while the other says "the water would be about the rail" (meaning, as I have since ascertained, the height of the highest deck) "when she made a sort of a stop, and then she gradually fell over." The point is one of very considerable importance (as indicative of the amount of stability actually possessed at the time), and I pressed my inquiries respecting it until I was satisfied that there was a slight return oscillation towards the upright; and this conclusion, while it is confirmed by a study of the ship's curve of stability and distribution of weights which have been prepared for the purposes of the inquiry, itself furnishes a check upon the deductions drawn from them.

No apprehension of the disaster having, so far as I can learn, existed before the launch, and no necessity for exceptional measures having been felt, no investigations or observations concerning the launching stability of the vessel were then made; it has, therefore, been a matter of great difficulty so to ascertain the facts and bring them together as to lay a solid foundation for this report. The difficulty has been increased by the circumstance of a discrepancy arising between the estimated draught of water and displacement of the ship as supplied by the builders, and the draught of water and displacement of the actual ship when floated on the 28th July. I have, however, taken the drawings of the ship handed to me in evidence by the builders as correct, because both the builders and the owners' overseer stated in their public evidence that the ship and the drawings in question corresponded; and after inspecting the vessel, and seeing how much weight had entered into her, I was in some degree prepared to find such a discrepancy. I have myself taken the actual draught of water of the ship, and ascertained by experiment (on the 28th July) the position of her centre of gravity. On all minor points, where the information could not be closely verified—such as the increased weight of the ship's woodwork caused by its long immersion and soaking in the river, and the precise positions at the time of the launch of some of the men and material on board—I have formed estimates as carefully as the nature of the case permitted. The experiment of the 28th July was performed in a sheltered dock, under conditions so favourable that I have perfect confidence in the result. Such loose weights as remained in the ship were placed, with slight exceptions, as nearly as possible in the positions they occupied at the time of the launch; the number of persons on board at the time was 21, and the ship was ballasted with 58 tons of ballast, of which 50 tons were in the holds, and 8 tons, with which the inclinations were effected, on the main deck. About 28 tons of loose materials which were in the ship at the launch (and have been either removed from her or recovered from the river, and carefully weighed) were left on shore at Linthouse, and, therefore, did not enter into the experiment. Under these conditions, I found that four tons of weight moved 20 feet across the deck inclined the ship 9 degrees 36 minutes ($9^{\circ} 36'$), and that the centre of gravity was situated at $10\frac{9}{10}$ inches below the metacentre. It now became necessary to pass from this condition of the vessel to that in which she was launched, and for this purpose the estimated weight of the water taken up by the woodwork of the ship, and of the mud that still clung more or less to the whole interior, especially in places difficult of access, had to be allowed for; all ballast had to be removed; the weights that had been landed and those recovered from the river had to be restored, together with the 195 workpeople who were on board at the launch, these being taken at an average weight of 10 stone. The effect of these operations (performed, of course, by careful calculation) was to reduce the above "metacentric height" (*i.e.*, height of metacentre above centre of gravity) from $10\frac{9}{10}$ inches to 4 inches.

I regard this metacentric height as much too small to ensure undoubted safety, especially with so considerable an amount of loose weights and men on board; but there are professional men who have hitherto regarded any amount of positive stability, however small, as sufficient for launching purposes, and I am not prepared to state that deficient metacentric height alone accounts for this accident, or that other vessels may not have been safely launched with no more metacentric height than she possessed. Ships are launched, and are sent to sea, as I shall presently show, with so very little metacentric stability that it is possible the "Daphne" might have remained safely afloat but for the fact that in her case very small initial stability appears to have been unexpectedly associated with such a surprisingly small growth of stability as she inclined that the power of recovering from an inclining or rolling movement which a ship

ordinarily possesses was largely wanting in her case. This disclosure is of the most important character, and reveals a state of things with which only scientific investigation can deal effectually, and which must be so dealt with if further disasters are to be avoided. It is now manifest that ships of modern type are sometimes characterised when floating light by a very abnormal deficiency of stability in inclined positions. I will explain the matter as it became developed during this inquiry.

The accident was so distressing to Messrs. Stephen, and cast upon them so many novel labours and anxieties, that they secured the services of Mr. Francis Elgar, member of the Institution of Naval Architects, for the investigation of the causes of the accident, and for furnishing me with such information from their firm as I desired. Mr. Elgar, having passed out of the Royal School of Naval Architecture with the highest honours, and subsequently acquired much practical experience, was well qualified for this duty, and entitled to have such representations as he made carefully considered.

In the course of his evidence, Mr. Elgar, on the 11th of July, gave the results of certain preliminary estimates and calculations which he had made for the "Daphne," and which showed that the ship appeared to have a metacentric height of nearly 8 inches. He went on to state that with this amount of stability, and with the usual height of side out of water, it had been most difficult to account (up to the date of his evidence) for the capsizing of the "Daphne." He further represented that it had been everywhere understood down to the present time that a ship having some actual stability in the upright position, and a high side, must of necessity be incapable of capsizing under all ordinary circumstances. So general was this belief that it had never, he said, occurred to anyone, so far as he was aware, "to calculate the actual curve of stability of a ship at her launching draught." The only exception to this known to him was the curve of the "Hammonia," which less than a year ago upon the Clyde nearly or quite capsized in launching. "The builders of that ship appear," said Mr. Elgar, "to have been as much astonished as Messrs. Stephen were in the case of the 'Daphne' at the ship having capsized, so they had the whole matter looked into, and they have had the actual curve of stability of that ship calculated;" and the belief of the witness was that this curve disclosed a deficiency of stability which had hitherto been wholly unexpected in such ships, and which it was quite impossible (the details of the "Hammonia" case being unknown to them) for the builders of the "Daphne" to have foreseen. This evidence of Mr. Elgar was substantially repeated by Mr. J. H. Biles, who also was a member of the Royal School of Naval Architecture, and who is now the naval architect to Messrs. J. and G. Thomson, of Clyde Bank. Mr. Biles produced the "Hammonia's" curve of stability, which he had himself calculated, and which disclosed the fact that, although the ship had some stability in the upright position, it nevertheless reached a *maximum* at about 33 degrees, vanished at about 50 degrees, and became again positive at about 90 degrees. Mr. Biles shared Mr. Elgar's belief that no previous curve of stability had ever been constructed for a mercantile ship at her launching draught. "I know of no calculation," he said, "for a complete curve of stability having been made, or even of the necessity of the thing having been suggested." I put to Mr. Biles the following question: "Are you of opinion that this case of the 'Hammonia,' taking it altogether apart from that of the 'Daphne,' discloses facts in connexion with the stability of ships in light launching condition that were not anticipated by yourself, or by any of those whom you are acquainted with?" And he answered, "I am quite of that opinion."

I should have been glad, had it been possible, to have avoided the necessity of dealing at length in this report with this very technical question, both because its investigation involves the introduction of somewhat abstract scientific matters, and because it is impossible to discuss it at all without making some reference to the stability of ships in other than launching conditions. But as the matter bears in the most direct and immediate manner upon the disaster to the "Daphne," and as the disclosure of unexpected circumstances concerning the launching condition of ships may serve to avert possible future calamities, I have felt bound to investigate the matter and report upon it.

Before proceeding further I would explain that the "curve of stability," to which reference has been made, is nothing more than a graphic representation of the successive leverages with which the buoyancy of the ship tends to force her either back into the upright position or further from it, when she has been by force inclined from that position. This leverage is measured by the distance between the two vertical lines through which the weight of the ship acts downward and her buoyancy acts upward respectively. As a given ship is more or less inclined, this distance or leverage usually alters, because the centre of buoyancy alters while the centre of gravity does not. These stability leverages, being calculated for various angles of inclination, are set up as ordinates along a base line

(at distances corresponding to the number of degrees in the respective angles), and a curve drawn through the ends of these ordinates becomes the "curve of stability." It is constructed, therefore, just as curves are being continually constructed to represent changes of temperature, changes of barometric pressure, growths of population, and a thousand other like varying quantities.

As no curve of stability has ever before been made public for a mercantile steamer *at launching draught*, and as I can find no trace of one having been made even in those shipbuilding establishments which are most advanced in scientific practice,* it may be proper to explain this deficiency by reminding you that the "curve of stability" is itself of quite recent construction. In 1867, on certain designs being submitted to the Admiralty which appeared to me manifestly deficient of stability at large angles of inclination, I had the stability calculated at several successive equal angles, and the results were brought to me in the graphic form of a curve.† Its construction was first publicly described, and the curve itself published, in a paper read by me at the Institution of Naval Architects in 1868. Since that time it has been universally employed for graphically illustrating both the statical and the dynamical stability of completed ships, but until the "Hammonia" case occurred it had not been produced apparently for any mercantile vessel in the launching condition, for the reason which I have quoted from the evidence of Mr. Elgar and Mr. Biles, viz., the general assumption that if a ship has initial stability and is high-sided, she can be launched with safety.

That this view has widely prevailed is quite certain. It has been taken very generally for granted that a high-sided ship with initial stability goes on gathering additional stability up to large angles, and that, although it reaches a maximum and then diminishes, it may be relied upon as existing in some degree even for very large angles of inclination. Sufficient initial stability has been regarded in ordinary ships as a guarantee of sufficient stability at all angles for launching purposes. For example, in reply to an inquiry of mine, Messrs. R. Napier and Sons of Glasgow, a firm of long standing and much eminence, say: "In vessels having a high side, and an unbroken side, the calculations for arriving at the metacentric height were held sufficient to satisfy everyone, and such a curve would have been considered superfluous." No firm upon the Clyde, or indeed anywhere else, have taken more pains, in my opinion, to apply scientific calculation to their work than Messrs. Denny Brothers, of Dumbarton, who have greatly distinguished themselves by organising at their establishment a costly scientific department and staff, and giving the profession the benefit of many of their investigations; yet Mr. Peter Denny (the senior member of the firm) says that curves of launching stability have not been made by him, and Mr. William Denny, in response to a written inquiry from me, says: "I know of no calculations for the stability of vessels in a launching condition which have gone beyond an estimate of the distance between the centre of gravity and metacentre. We ourselves have never done more than this. We have never got out curves of range of stability for a launch, nor do I know of anyone who has done so, excepting the case of the 'Hammonia,' in which, however, they were carried out after the launch." This view has taken another form in the literature of Naval Architecture, where it has been laid down that if a ship has initial stability, and has some stability also at very large angles of inclination, say 90 degrees, then it is quite certain that she will possess stability at all intermediate angles of inclination. This principle was laid down quite clearly in an able paper "On the Calculation of the Stability of Ships, and some matters of interest connected therewith," read at the Institution of Naval Architects in 1871, and has been repeated in Mr. W. H. White's "Manual of Naval Architecture." The "Hammonia's" case has clearly shown, however, that the stability at the large angle of inclination, instead of being a continuation of the positive stability of the upright position, may be a re-appearance of positive stability occurring after an interval of instability, and consequently that the existence of positive stability, both at the upright position and at extreme angles of inclination, is no guarantee whatever of safety, at least in certain cases; nor can it at all be assumed hereafter that, because a high-sided vessel has initial stability, she can be relied upon not to capsize under ordinary launching conditions. It is not easy to see how the doctrines which I am here calling in question came to be widely accepted by the profession. They will not for a single moment stand the test of scientific investigation; as soon as they are carefully considered they are seen to be wrong. They are, at the best, but specious generalisations from a

* A remark of Mr. Pearce (John Elder and Co.), made during the giving of his evidence at my inquiry, would seem to conflict with his statement, but I have since learnt that Mr. Pearce had in his mind, at the moment, calculations of initial stability and not of curves of stability.

† Mr. N. Barnaby, C.B., the present Director of Naval Construction at the Admiralty, was the officer who conducted, and Mr. W. John, now the manager of the Shipbuilding Works at Barrow, was the officer who actually made the calculations, and produced this, the first "curve of stability" ever constructed.

limited class of familiar facts, and though they may find many seeming corroborations under the ordinary conditions of ships, they may also be disproved by abundant examples. It is needless even to draw such examples from ships, for the most elementary forms of floating bodies furnish ready illustrations. In the 1871 paper, "On the Calculation of the Stability of Ships," to which I have just adverted, floating bodies of square sections and parallel throughout their length are employed as illustrations of certain scientific principles. Now a floating body of this simple description serves perfectly to illustrate the unsoundness of the doctrines in question. I have ascertained the stability of such a body of square section, immersed at various depths, and have constructed for it curves of stability corresponding to different positions of the centre of gravity, and have thus obtained numerous confutations of the principle. A single case will here suffice. When this body is immersed $\frac{3}{5}$ ths of its depth, and has its centre of gravity two feet below its centre of form, it has abundant stability both in the upright position and inclined at 90 degrees, but none at all when inclined at 66 degrees. When the centre of gravity is raised a foot it still has large stability upright and good stability at 90 degrees, but it loses all stability at the intermediate inclination of 53 degrees, and only begins to regain it at 82 degrees. With its centre of gravity at its centre of form (and same immersion as before) it has good stability upright, but loses all stability at 45 degrees, regaining it only at 90.

The "Hammonia" example shows that the doctrine is no more true for ships than for prismatic forms; and it consequently follows that professional men must forthwith abandon a doctrine which they have, to a very large extent at least, accepted without question, but which is now seen to involve public danger.

Now, although in the "Daphne" case there is no recovery of stability at 90 degrees as in the "Hammonia,"—a condition which may or may not be associated with the other characteristics of ship-stability at very light draughts, and which much depends upon the form of the upper portion of the ship,—and although the actual metacentric height of the "Daphne," as launched, was less than Mr. Elgar estimated it approximately at the early stage of the inquiry, it is manifest that the present disaster was much influenced in its character, and the complete capsizing brought about, by the unexpectedly slow growth of the stability already adverted to. I have had the ship's curve of stability at the launching draught carefully calculated, and it appears that as she inclined she gathered stability so slowly that at an angle of inclination of 10 degrees her lever of stability was only about $\frac{3}{4}$ of an inch in length, giving a righting "moment" of only about $32\frac{1}{2}$ foot-tons; at an angle of 20 degrees the lever was $2\frac{1}{8}$ inches long, and the righting moment 89 foot-tons; at 30 degrees the lever was $4\frac{3}{8}$ inches, and the righting moment 182 foot-tons. These increases of stability are so small that when first brought to light by the calculations made after my experiment upon the ship on the 28th July, they seemed open to doubt, notwithstanding the fact that curves previously calculated in London, the evidence of Mr. Elgar, and the case of the "Hammonia" had all made a specially low curve of stability seem probable. Mr. Peter Denny, of Dumbarton, in whose establishment ample appliances for ship-building calculations exist, as I have previously intimated, having kindly placed them at my disposal, I had the curve of stability for the launching condition of the ship calculated there, and as the results conformed closely to other and independently-constructed curves, there appears to be no reason to doubt the accuracy of the work upon which the above figures are based, notwithstanding their very unusual character. I will complete the figures by saying that, presuming all openings to have been effectually closed during the inclination of the ship, and the whole ship watertight up to the top deck, also that no weights shifted, the lever of stability at 40 degrees of inclination would have been $7\frac{1}{4}$ inches, and the moment of stability 315 foot-tons; at 50 degrees the lever would be nearly 1 foot in length, and the righting moment 500 foot-tons. At $50\frac{1}{2}$ degrees the whole side would be immersed, and all stability would be gone. It must be perfectly obvious to everyone, however, that long before the ship could reach this inclination the men and much of the loose materials must have begun to slip down the decks, and have introduced altogether new conditions. I have made numerous calculations showing what changed conditions would exist upon various assumptions as to weights moved; but although there were over 30 tons of loose weights (including men) on board, I will content myself with assuming that only 15 tons moved across the decks and holds, through an average distance of 15 feet,—a moderate assumption. This, however, would have sufficed to incline the ship permanently at an angle of over 30 degrees from the upright, and in carrying her over to that angle would have accumulated sufficient work to nearly, but not quite, absorb the remaining stability. The vessel would, on this assumption, have ceased to roll over, and even have returned slightly towards the upright position; but it is

not to be expected that the vessel was water-tight up to the top deck; indeed it is certain that she was not, and that some water found its way on to the main deck, and this fact, taken together with the further shifting of weights, which would occur at so large an inclination, would cause her to be carried over again. At $50\frac{1}{2}$ degrees the water would flow over the bulwarks, and complete the capsize. It is unnecessary to enter here into minor particulars, because it will be obvious that, with so little increase of stability as she inclined, the capsizing of the vessel is clearly accounted for. Before passing on, however, it may be well to exhibit the contrast between the "Daphne" and other vessels of small initial stability; and for this purpose I have taken almost at random, and as ordinary examples, two other mercantile steamers, which I will call A and B, and of which the curves of stability are known, and reducing them as well as the "Daphne" to one standard, by bringing the metacentric height of all to zero, I obtain the following lengths of stability levers at different angles, observing that in this comparison I assume that the bulwarks above the main deck and the poop are not water-tight, and that the stability ceases with the gradual immersion of the main deck:—

		At 10 degrees.	At 20 degrees.	At 30 degrees.	At 40 degrees.	At 50 degrees.	At 60 degrees.
"Daphne" -		inches. $\frac{1}{18}$	inches. $\frac{2}{3}$	inches. $2\frac{1}{4}$	inches. $1\frac{1}{8}$	inches. $-1\frac{1}{8}$	inches. -7
Ship A - -		$\frac{1}{8}$	$1\frac{7}{8}$	$6\frac{1}{8}$	$11\frac{1}{8}$	$13\frac{7}{8}$	12
Ship B - -		$\frac{1}{4}$	$2\frac{1}{8}$	$5\frac{7}{8}$	$10\frac{9}{16}$	$12\frac{1}{8}$	$11\frac{3}{8}$

Were I to regard this Clyde disaster in the abstract, or to make my own sense of what is right and safe at ship launches my only standard, I should certainly say that before launching the "Daphne," with so many men and so much loose material on board, the builders should, even without any suggestion as to a slow and small increase of stability as the vessel inclined, have calculated the initial stability of the vessel, and finding it as small as it was, as they should have done, they should have materially altered the conditions of the launch. But I cannot, with due regard to what is just, fail to qualify this statement by pointing out the relation of this case to the general practice of ship-builders and shipowners, and to the grave defect in the assumed theory of the stability of ships at light draught which, as we have seen, this disaster has been the means of disclosing. That there has been a great disclosure, and one which during the three weeks this inquiry lasted already began to have effect, is manifest. On the 24th July Mr. William Denny, in the letter already quoted, says, "The cases of the 'Hammonia' and 'Daphne' show the necessity of the more elaborate investigation, but the need for it was not apparent to me before learning of them," and after explaining the highly commendable practice of the firm with respect to the detailed calculations of the stability of ships, he adds, "Undoubtedly the cases of the 'Hammonia' and 'Daphne' will lead to our extending our practice for each individual steamer." Messrs. R. Napier and Son, on the 28th July say, "The evidence given before you, Sir, as regards the 'Hammonia' showing that * * * the stability may rapidly disappear even in high-sided ships when inclined, has caused us to make a standing order that in future a stability curve is to be run out before launching any ship." On the 4th of August, Mr. Wm. Pearce, whose high position and large experience I have previously adverted to, and who has carried the construction of fast sea-going ships (in the "Alaska" and other Atlantic mail boats) almost beyond present competition, sent me the following important letter: "With reference to the case of the 'Hammonia,' and so far as can at present be seen to that of the 'Daphne,' it may be well for you to know that some addition to the calculations made before launching ships appears, in my judgment, to be desirable in certain cases. Hitherto it has been my practice to have the metacentric height run out before launching for all ships about which any question of launching stability could arise, and wherever the metacentric height was not large and ample I have put ballast on board to increase it. In future, in all cases where the necessity for care arises, I shall cause a curve of stability likewise to be run out, so that the stability at considerable angles of inclination, as well as the initial stability, may be brought fully into view. In some cases this may lead to the use of ballast as at present, but in others it may show, by a full curve, that ballast is

"needless." I could multiply instances of this kind; but when such shipbuilders as Mr. Pearce, Messrs. R. Napier and Sons, and Messrs. Denny, without hesitation or delay, proceed to act upon a novel development of professional science, its importance cannot be doubted, nor its adoption be long neglected by others.

The general belief that a high-sided ship having some initial stability will, as she inclines, gather large additional stability, and will retain some even at very large angles, has exercised a widespread influence on modern mercantile shipbuilding, and has greatly encouraged people to be satisfied with very small initial stability, in some cases with none at all, and even less than none. Many steamships of large tonnage have been built of late years, for influential steam companies and other owners, which ships are wholly incapable of floating upright without the aid of ballast or of cargo, and which cannot be unloaded in dock without being held upright by hawsers attached to the shore. Such ships, even when capable of floating unballasted without capsizing, can only do so by lolling over at large angles of inclination, and there finding a position of stable equilibrium. When carefully watched over, and stowed with suitable cargoes, these ships can usually be made safe at sea, and sometimes even safer than ships with larger initial stability but less range,—a circumstance to which undue prominence has perhaps been given, and which has diverted many from the grave elements of danger which much more often are associated with small initial stability. There is not the least doubt, however, that the very small initial stability given to many modern mercantile steamships—given in the belief that much more is sure to be rapidly gained as the ship inclines (within large limits)—has resulted in the capsizing of many ships at sea, and in grave danger to many that are still afloat, not in the same manner, because not in the same condition as to lightness, as the "Hammonia" and "Daphne," but from other not less real deficiencies. Sometimes such vessels are brought into a condition of apparent safety by the stowage of their own coal; but as the coal is consumed their stability diminishes, they capsize, disappear, and the word "Missing" is recorded against them in an official return. No means exist, notwithstanding all our shipping legislation, for ensuring that the facts will be brought to light; indeed, at the official inquiry which follows, under present conditions the question of stability may not even be mentioned. As the stability of a ship is often an intricate matter, which can be effectually controlled only by close and careful calculation, and as no Government department is at present charged with the duty even of collecting, recording, and making known those dimensions and particulars of ships which determine their stability, the matter must be left to right itself. Meantime ships of small stability incur dangers from, and are doubtless lost by the operation of, causes which are but very imperfectly appreciated. One of these is the meeting with waves that are synchronous with the ship's period. As long ago as 1860, in the very first paper read at the Institution of Naval Architects—a paper which may be truly said to have opened up a new era in naval science—its distinguished author, Dr. Joseph Woolley (who afterwards became the Admiralty Director of Education, and in many ways gave development to the scientific period which his earlier labours opened), said, "We can easily conceive how, in a storm at sea, a succession of blows (or wave impulses) synchronising with the oscillations of the vessel might become dangerous and even fatal." This suggestion has since been greatly expanded, and the danger of accumulated rolling has become well understood, but only by those who make a study of such subjects. Another more subtle, but probably more common, element of danger to ships of small stability is the temporary loss of gravity, and consequent loss of stability, due to the upheaval of ships at sea by the waves. "We are accustomed in discussions on rolling to neglect the alterations in what we call the 'virtual weight' of the ship due to heaving motion. But when you come to the case of ships at sea heaving up and down on waves 25 feet high or more, it is a mathematical demonstration, as well as a matter confirmed by actual measurements on board ship, that you may have a variation of one-fourth or one-fifth in the virtual weight as compared with the actual weight. On the wave crest the virtual weight may be roughly about 20 per cent. less than the actual weight would be. If the ship is acted upon by external forces, like the wind pressures, which are unaffected by the wave motions, and her moment of stability is lessened by such a decrease in the virtual weight, there occurs an enormous increase of heel, almost instantaneously, in consequence of the defect in stability."* This is a cause of danger which would usually take its greatest effect in sailing ships when upon the summit of a large wave; but the same cause is at work, in so far as diminished stability is concerned, upon all ships alike, and where the stability of a

* From a speech by Mr. White in the Transactions of the Institution of Naval Architects, 1877, Vol. XVIII., p. 210.

steamship at sea is very small, its reduction due to the upheaval of the sea may be quite sufficient, in co-operation with other causes, to compromise and sacrifice her.

It is unnecessary to speak at any length of the ordinary and well-known dangers resulting from low freeboards, shifting cargoes, and other like causes, in ships of small stability. It will suffice to quote here, first a remark of Mr. Benjamin Martell, chief surveyor of Lloyd's Register Office, who in 1880, speaking of certain types of merchant steamers, said, "It may be said almost with certainty that many of these steamers combine in themselves many of the worst features which contribute to instability and consequent unsafety;" and next, a remark made publicly by Mr. W. John, a naval architect who has distinguished himself in the Admiralty, in Lloyd's Register Office, and at the Barrow-in-Furness Shipbuilding Works. Mr. John, who (as I have stated in a previous foot-note) first put the stability leverages of a ship into the form of a curve of stability, and who, when at Lloyd's, devoted himself very earnestly to the question of merchant-ship stability, said at the Institution of Naval Architects in 1881, "I believe, most sincerely, that the great proportion of disasters at sea is due to want of stability."

It being impossible to separate the "Daphne" case from this more general deficiency of stability in mercantile steamers,—because the "Daphne," with her high weights and low metacentre, which are inherent in her, is a vessel of small natural stability, and will usually, I should think, need the aid of ballast, or of heavy cargoes stowed low,—the question arises whether the frequent resort to vessels of small stability is due to any commercial necessity, or even to the desire of commercial advantage. Now, undoubtedly, it was at one time a very widely-accepted belief among shipowners and shipbuilders that vessels of small proportionate beam or breadth were more economically propelled at sea. The belief was, in large part, erroneous, and has been more or less abandoned, and it may be broadly said that neither does commercial necessity require nor commercial advantage result from the adoption of proportions involving instability. There are exceptions, but this is the rule. I find it fully admitted in the *Shipping and Mercantile Gazette*, in the leading columns of which, in December last, an apparently skilled writer said, "It is difficult to imagine what motives can influence persons responsible for the construction of ships to fix such dimensions and cause them to be so deficient in stability. There is no reason why it should be so, and no gain in any other direction. If the vessels could be improved in speed or carrying capacity by reducing their stability, there might be some show of excuse for it, although even then such advantages would certainly have been far too dearly bought. But this is not the case. It is an old-fashioned but long since exploded notion that smallness of beam is favourable to speed; but we assert without hesitation that these ships could have been designed with sufficient beam to give them ample and unquestionable stability under all possible conditions; to have had the same carrying capacity, and at least equal speed, with the same power. One of the most essential qualities to a ship's safety, efficiency, and economy, viz., stability, has been needlessly sacrificed."

But the truth is that frequently it is not upon any defined principle that the proportions of mercantile steamships are fixed, nor are questions of stability much considered when the order is given. It is a matter of complaint with the best shipbuilders that very often but little time is allowed the builder before closing a contract, and also that owners take upon themselves the responsibility of determining the dimensions and proportions of the ship. On this latter point Mr. John Inglis, junior,* of the eminent shipbuilding firm of A. and J. Inglis, of Pointhouse, Glasgow, may be cited. "I think," he said in London in 1881, "If there were more relation between the shipowner and the shipbuilder in regard to these questions, if the shipbuilder were more consulted than he is, the result would be a great improvement of merchant ships generally, especially in this respect. The shipbuilder, in point of fact, is not often consulted even with regard to the leading dimensions of the ship. He gets his printed specification, where everything is set forth, and his opinion is never asked as to whether the dimensions are good or bad for the purpose intended." Apart from these remarks, and before they were made, Mr. William Denny, speaking of cases in which the builder is allowed to have some influence upon the character of a ship, said, "Very frequently we have, in contracting for a vessel, to settle her general conditions and character within, at most, the limit of one day." My own experience confirms the evidence of both these gentlemen, and

* I cannot mention here the name of this gentleman without observing that by the scientific spirit which he has displayed in his work, and by the efforts which he has made in a very unostentatious manner to bring the qualities and conditions of merchant ships within the reach of owners and captains, he has earned the gratitude alike of builders and owners of ships.

the "Daphne" case furnishes an illustration of the manner in which mercantile steamers often, much too often, come to be what they are. The specification of the ship was prepared and sent to the contracting builders by the "Glasgow and Londonderry Steam Packet Company," for whom the vessel was built. The specification was based upon the experience of the company, and rested in no degree upon any exact calculations whatever, so far as I have been able to discover. I asked Mr. A. R. Brand, who obligingly attended the inquiry on behalf of the managing owners of the company, "Is it your practice as a company to leave the responsibility of investigating designs of ships solely to the examination of a marine engineer, or to a gentleman not trained in any way as a naval architect?" Mr. Brand replied, "It has hitherto been the practice of the company to do so. Their vessels have all been built to a specification that has been prepared by the engineer, and it is sent out to various shipbuilders and tendered upon." I further asked, "Do you feel quite justified, as shipowners, in ordering ships and specifying dimensions and sizes, and almost every element of a ship, without taking any advice whatever from any man who has ever been trained to shipbuilding, or to the art and science of shipbuilding?" And Mr. Brand answered, with honourable candour, "We have done so hitherto." Mr. W. Laing, marine engineer, who acted as the adviser of the owners in this case, fixing the dimensions of the vessel, defining the displacement, and determining the principal distribution of weights by means of a specification, stated that he had made no calculation of stability for the "Daphne;" indeed, that he had "never made any study of the stability of ships," or "very little at all events"; that he did not consider himself competent to determine the elements of a ship's stability. From the questions put to this witness I extract the two following, with his replies: (Qu. 836.) "Did it not occur to you in fixing the dimensions of the vessel, and approving of drawings and sections and so on, that in putting these very heavy deckhouses and hatchway coamings and lead coverings to the elevated deckhouses and other fittings, you were accumulating a very large amount of weight on the upper deck of such a vessel, and that you were dealing with the question of stability in a serious way?"—"No, I never doubted it, because she has a much higher proportion of breadth of beam than any vessels we have hitherto had except the 'Briar.' Of course the 'Briar' is a larger vessel, but I thought that was quite sufficient." (Qu. 837.) "In other words, the question of stability has mainly been got at in this case, as in many others, by 'experience,' and you had no suspicion whatever that it was necessary in determining all these things to make any special investigation of stability?"—"No, I never had the slightest doubt or thought in my mind about it." We have here the illustration of the system previously described in the language of Mr. John Inglis, junior, and which system is far more general throughout the shipbuilding ports than the outside world would readily believe. One would suppose that a construction like a ship, built expressly to float in so yielding a material as water, and to be subjected to all its worst and most stormy disturbances, would in every case, without a single exception, receive whatever benefit the available skill and care of its authors could provide. But, somehow, men come to regard the matter as a very commonplace one, and, guiding themselves by "experience," consider they can pass from ship to ship with perfect safety, modifying dimensions, adding decks and deckhouses, adopting novel and elevated machinery for dealing with cargo, putting deck fittings big enough for a large ship into a small one, without ever calculating or even reflecting upon what is to be the effect of all this upon the ship's capabilities. Meanwhile the sea does not grow one whit more forbearing or accommodating than it ever was; "gravity" has its grasp and downward drag upon every part; and "buoyancy" pushes upward with equal force and constancy. These forces of nature, ever present and ever ready to work their way upon the ship, may indeed be controlled and made ministers of safety, but their control can only come from knowledge and understanding of them, to which "experience" no doubt vastly contributes. But experience alone cannot meet the requirements of the case without disasters; indeed disasters are an essential part of the lessons which experience depends upon, and accidents, or at least narrow escapes from accident, are the only means by which the errors of those who do not seek scientific guidance can be corrected.* I am obliged to insist upon this in the present case because the "Daphne" accident (apart

* We have previously seen that even with scientific guidance, and under its direction, grave errors may be committed in matters so comparatively novel, and so little developed, as the stability of steamships under exceptional conditions; but the occasional imperfection of science should urge us to extend and improve it, and cannot in any degree justify the neglect of what is settled beyond doubt or question. We are having abundant proofs at present that scientific knowledge is absolutely indispensable to the solution of the many difficult and various problems which ships of small stability are forcing upon builders and owners, and more especially upon the latter.

altogether from the newly-disclosed deficiency of stability in some vessels at light draught) furnishes a manifest and public illustration of that which is often silently illustrated by the disappearance of "missing" ships, viz., the insensible decrease of a ship's stability brought about by gradual increases of top-weights. The "Daphne" is a small vessel, never when fully laden to reach to 1,000 tons of displacement, yet she is fitted with a poop and forecastle and an upper or spar deck amidships, and upon the top of these stand heavy iron deckhouses, hatchways, and companions, two heavy steam winches, a steam windlass, a steam-steering engine, a quadrant and hand-steering apparatus large enough for a vessel twice the size, a large number of very heavy iron castings to serve as bollards, fair-leads, &c., and in addition to all this, heavy boat davits and sockets for lifting the boats high above everything on the deck, with lofty wrought-iron double frames to support the boats in their elevated position. I estimate the weight of these high-raised equipments, including the additional weight of hard deck planks and chocks to support the winches, windlasses, &c. (but without the boats), to exceed 30 tons, which could only be balanced by about 50 tons of ballast stowed as low down in the hold as possible. I cannot myself look along the vessel's deck without at once thinking of the effect of so much top-weight upon the stability of so small a vessel; but the engineer who specified all these things had specified somewhat similar ones for previous ships, not very much larger than the "Daphne," and as none of these ships had capsized, or given him cause for dissatisfaction or anxiety, his "experience" furnished no suggestion of danger, and he doubtless said with perfect truth, "I never had the slightest doubt or thought in my mind about it." And yet had some of these upper weights been omitted, as they certainly might have been without detriment, and had larger initial stability thus have been obtained, it might well have happened that, notwithstanding the unexpected character of her later stability, the effort of the ship to recover herself *might* have been successful. I trust that this disaster, followed by this statement of the facts, may be carefully pondered by those throughout the shipbuilding and shipowning world who have only "experience" to rely upon, and will suggest the necessity for at least supplementing experience by those investigations and calculations which the settled science of the time approves, and which the energy and prescience of not a few "Clyde shipbuilders" have helped to place within the means of all who build ships.

We have now seen that the "Daphne" had very small initial stability in the launching condition; that this was attended, very unexpectedly to all concerned, by the absence of the usual power of rapidly acquiring stability as she inclined; that the smallness of the initial stability was in part due to the presence on the bridge-deck, poop, and forecastle of very heavy equipments, and the consequent raising of the centre of gravity; that there were many loose materials of considerable weight on board, and also many workpeople, by the shifting of some of which a new demand was made upon the stability when the ship inclined considerably; and to these facts may be added the further one that the engines (but not the boilers) were secured on board when the launch took place. It becomes necessary to add a few remarks upon some of these facts.

Taking the last of them first, I would point out that it is the usual practice at the works of Messrs. Stephen to launch their ships not only with the engines but with the boilers on board. Having no basin or wet dock of their own (few builders have), but having engine works at Linthouse, it is convenient and natural for them to pursue this practice, and certainly, in the large majority of cases, there is no sort of danger incurred by it. In the present case, the effect of the machinery (including the propeller and shafting) being on board was to lower the centre of gravity of the whole ship and contents 3-10ths of a foot, or $3\frac{1}{2}$ inches; but as the metacentre was lowered about 4 inches, the metacentric height was reduced by half an inch. On the other hand, the weight of the whole ship acting upon the righting lever was increased in the proportion of from 45 to 52, and, consequently, the moment of stability was, at all angles of inclination, 1-16th greater with the engines on board than it would have been without them. The presence of the engines was, therefore, on the whole, advantageous to the "Daphne."* It may be worth while to add that we have here an illustration of the necessity for close and careful regard to all the facts in order to determine the full effect upon the stability of the introduction of weights into a ship. The centre of gravity might, by chance, have been higher or lower, and the metacentric curve might have been of a different form; so that either a considerable increase or a considerable loss of stability was therefore possible, and is always possible, by the placing of the engines on

* It has been suggested that the weight of the engines largely preponderated to the port side. As a matter of fact, they were almost evenly balanced.

board a ship. It is only by bringing all the elements of weights, centres of gravity, and metacentric curves into their due relations that the result can be truly determined.

With regard to the number of men and quantity of unfixed materials on board, there can be no doubt that these were large for the size of the ship; but I can find no evidence of any departure in this case from the usual practice of the firm, which had been pursued with success in many previous ships. As regards the men and boys on board, although I have taken great pains to invite information on the point, not a single one of those saved, and no relative of anyone deceased, has suggested that they were there by any kind of compulsion, or under any special order, or by special desire on the part of the builders. They happened to be at work upon the ship, and they remained on board, and in many cases were actually at their work in different parts of the ship when she was launched, as the positions in which their bodies were found clearly showed. Only a single stranger was on the ship at the time, and he was taken on board by a workman. The number of people on board certainly seemed to me culpably large when I first began this investigation, and when the facts were imperfectly known. Had they been strangers specially brought on board by heedless invitation, or workmen crowded there to earn a premium, or avoid contract penalties, or to bring about some pecuniary advantage, blame might readily have been imputed. But the evidence has shown that no excessive pressure as to time of completion was put upon the builders, and there was much of the contract time still unexpired. The fact appears to be that the ship was in that state of advancement in which a great variety of work was in hand, and as she was to be removed from Linthouse to the Broomielaw, at a distance from the works, it was convenient to all parties to finish her off quickly. The workmen themselves seem to have pressed on board more than the builders desired, and the officers of the builders seem to have endeavoured to prevent them from doing so, the following evidence of Mr. McMaster, their out-door manager, given publicly on the 11th of July, remaining entirely uncontradicted till the close of the inquiry on the 31st July:—"The only thing that I myself did was, there were a number of men and boys who were not started to their work that day, and I went up to the head of the gangway and drove every man down from the head to the bottom. They said they were going on board the ship to work, and I said, 'I do not care where you work, you can travel to the quay,' because there were a number of hands all going on board purposely to get the passage up on board the ship, and I drove all and sundry down once or twice."

As regards the loose materials on board, they were placed there in the ordinary course of business, in order to be fixed as the work progressed, and nothing appears to have suggested to anyone concerned their possible interference with the vessel's stability or safety. The necessity of closing the side scuttles, cattle ports, and other openings through the sides of the vessel was well understood and appreciated, and attended to with care, excepting certain small water ports for clearing the main deck of water, about which there is some doubt. It would have been well to have made certain that they also were closed, but they were too few and too small to have any serious influence upon the accident, especially as they were above the main deck.

Before proceeding to Part II. of this report, it may be proper to express—as I understand I am expected by you to do—the view I take of the responsibilities involved in this disaster, and I would say:—

First, I am thoroughly convinced that neither the builders nor the owners of this ship, nor any persons in their service, were in the least degree conscious that any exceptional risk was being incurred by the "Daphne's" launch, or in the least degree negligent of any precaution which they knew or believed to be essential to its success and safety. Apart from the question of the vessel's stability, everything that experience could suggest to secure a safe launch was attended to, and all the arrangements were carried out satisfactorily. The manner in which both builders and owners have assisted in giving fulness and completeness to the official inquiry is most honourable to them, and suggests complete unconsciousness of any intentional or known negligence on their part.

Secondly, I am equally convinced that the system which was pursued at the origin of this vessel, under which owners define so many dimensions and particulars of a ship as to leave the designer but extremely little scope for the determination of the elements which compose or regulate a vessel's stability, and under which system builders conform to these requirements of owners, and undertake to carry them out without a full investigation of that subject, is essentially a bad and dangerous one, bringing about a division and confusion of responsibility, and opening avenues to accidents which neither party foresees. I am also of opinion that the system, which likewise has been pursued in this case, of entrusting the overseeing of ships throughout their construction and equipment

exclusively to persons who are unable to judge of the effects which the enforcement of their requirements may produce upon stability and other vital qualities is, although in a less degree, an essentially bad and dangerous one; but both of these systems are so widely practised, and receive the sanction of so many eminent owners and builders, that I should regard it as a harsh and more or less unjust use of the "Daphne" disaster to visit its owners and builders with the consequences, even if no unforeseen and exceptional condition of the ship as regards stability had been developed.

Thirdly, it would have been well, doubtless, as the event has proved, for the builders of the "Daphne" to have made an exception of this vessel, by calculating before the launch, not only the height of her metacentre, which is often done, but also, as approximately as was possible, the height of the centre of gravity, which is much less often done before launching. I am of opinion, however, that no such calculations as would usually be made either at Linthouse or at many scores of other private shipbuilding establishments would have been likely to develop the real state of the ship: first, because much of the excess weights in the upper parts and equipments of the ship would probably have escaped detection in any merely approximate calculations, conducted by those who had no suspicion of risk or danger; in the second place, because very small initial stability had come to be widely regarded as sufficient for practical purposes; and in the third place, because there lay behind the question of metacentric height the undisclosed question of very small angular stability, which in this case proved so fatally deficient. I regard the two successive elements of initial and subsequent stability as inseparably connected in this case, which has proved even more exceptional than that of the "Hammonia," yet in the "Hammonia" case Mr. Biles stated in his evidence that he had very carefully calculated the metacentric height of the "Hammonia," and "on the face of it there did not appear to be any reason why the ship should turn over." He also said, "*Until we had that experience we should certainly have said that the ship was quite safe to launch with any positive stability at all.*" The "Daphne" had some positive stability, and the same conclusion might have been drawn in her case by those who knew nothing of the fatal smallness of the stability to be developed during inclination, as subsequently shown by my calculations. I cannot presume for a moment to put the question upon legal ground, but I may be permitted to quote from the *Times* of Tuesday, July 31st, an extract from a written judgment of the Master of the Rolls in the case of *Heaven v. Pender*. In this passage the learned judge lays down a broad and general proposition defining responsibility where the question of negligence is possibly involved, and the proposition is this:—

"Whenever one person is by circumstances placed in such a position with regard to another that everyone of ordinary sense who did think would at once recognise that if he did not use ordinary care and skill in his own conduct with regard to those circumstances he would cause danger of injury to the person or property of the other, a duty arises to use ordinary care and skill to avoid such danger." Now, in the present case, it has been shown that no ordinary care and skill in the conduct of the builders or persons in their service could have been expected to provide against the extraordinarily slow development of stability which the "curve" of this ship's stability in launching trim has so unexpectedly unfolded; and as the complete capsizing of the vessel (notwithstanding the presence of the men and loose materials) is due, in my opinion, to this slow development of stability,—seeing that, even slow as its development was, the ship nearly recovered herself by its means,—I am of opinion that no duty arose in this case which can properly be said to have been neglected. I therefore regard the builders as blameless of wrong or negligence under the peculiar circumstances of this accident, and in view of the disclosure in the matter of stability to which it has led.

Holding the above opinions as regards responsibility in the present case, I must nevertheless express the belief that, when the facts of the case become known, a new responsibility will be felt throughout the mercantile marine. The lives lost by the capsizing of the "Daphne" will not have been lost in vain if they arouse shipowners and shipbuilders generally to the fact that the stability of their ships is a subject which deserves vastly more consideration than it at present receives. Owners should cease to arrange and settle all those things which determine the ship's stability, unless they qualify themselves for the work, or employ competent advisers. Shipbuilders, who now too often are ready to build and launch whatever owners demand, should remember that, in the launching condition at least, the confidence hitherto felt in initial stability and metacentric height,—in "any positive stability at all," as Mr. Biles put the matter,—may prove a fatal snare and delusion.

I venture to add (in view of the relation of this case to the general question of stability) that with a large number of ships afloat possessing so little stability as to make their safety at sea dependent upon the judicious stowage of cargo, arrangement of coal, and use of water ballast, it would appear to be high time that some knowledge of the elements of a ship's stability were imparted to the officers of the mercantile marine, and required of them by the Board of Trade examinations. I must be excused for further adding that the recent legislation of Parliament has, in my opinion, been much less efficient in preserving life and property at sea than it might have been had the Board of Trade understood and recognised the very large extent to which this question of "stability" of necessity enters into the design, construction, stowage, load-line freeboard, and almost every other subject with which the Board has to deal in regulating merchant-shipping matters.

During the inquiry at Glasgow a question arose respecting the existing regulations of the Clyde Trust with regard to the controlling of the traffic during the launching of ships, the Clyde being a narrow river teeming upon both its banks with shipbuilding establishments. In the existing "Bye-laws and Regulations enacted by the Trustees of the Clyde Navigation," dated April 23, 1860, there is nothing by which the launching of vessels into the river is in any way regulated. In 1878, however, an attempt was made by the Clyde Trustees to get a new set of byelaws and regulations passed. They were submitted to the sheriffs of the three counties through which the jurisdiction of the Clyde Trust extends, the approval of the sheriffs being necessary before the new bye-laws could come into force. One of the proposed regulations was framed for the purpose of dealing with launches, and is as follows:—"Every master or other person in charge of any vessel passing or being on the river in the vicinity of any ship-launch shall observe the signal exhibited to give notice of such a launch, which shall be a red flag, with the word 'LAUNCH' in large white letters thereon, displayed from a boat on the river, and shall not pass such signal until the launch has taken place or the signal be withdrawn."

The Trustees also sought to provide that any violation of this law would be attended with a penalty.

The sheriffs of the three counties refused their sanction for reasons which had no connexion with the ship-launching question. But although the new byelaws never came into force, the shipbuilders generally appear to have acted upon the intimation embodied in the proposed bye-law, and have complied with the requirements as to signals therein set forth; but it is alleged that in more than one instance the danger flags exhibited by them from boats up and down the river have been disregarded, and vessels, when being launched, have consequently been in imminent danger of causing accidents more or less serious in their consequences. In one case an accident occurred, and led to criminal proceedings being instituted.

From what has been previously said in this report upon the causes of the "Daphne" disaster, it is obvious that it has but a remote relation to this question of the launching regulations, and it is unnecessary for me to enlarge upon it. The matter is, however, so serious in itself that I think it right to append hereto the evidence and correspondence respecting it, and to say that the existing want of understanding between some of the Clyde shipbuilding firms and the Clyde Trustees, and the absence of authoritative bye-laws and regulations to control the traffic of the river during ship-launches, appear to me likely to lead to accidents such as might be attended by loss of life. I, therefore, recommend to your notice the desirability of effecting some substantial improvement upon the present arrangements.



PART II.

It may be well to commence this part of the report by illustrating the manner in which the curve of stability is constructed. Fig. 1 may be taken as the transverse section of a vessel, inclined at an angle of 15 degrees to the upright. The total weight or gravity of the vessel will act downwards through the centre of gravity G , and the total buoyancy will act upwards through the centre of buoyancy B , as the arrows indicate. It will be obvious that whether we regard either the one or the other (the weight or the buoyancy) as predominating, or both as operating equally, the result will be the same, viz., to push the vessel back to the upright position. She obviously cannot rest in the inclined position with these forces and no other operating upon her; she must revolve until gravity and buoyancy act in the same vertical line, but in opposite directions. If she oscillates back and passes the upright position, there will at once be an excess of buoyancy on the opposite side; the centre of buoyancy will move out, and she will be pushed back again to the upright. It is also obvious that the further she is inclined the more will the ship be immersed on one side and emerged on the other, and therefore the further out will the centre of buoyancy move. Now as neither the gravity nor the buoyancy need be altered in amount by mere inclination, and as they are equal and operating equally, it follows that, whatever the inclination, the force acting will be always the same, but the leverage marked GZ will vary as the centre of buoyancy moves. At 30 degrees inclination, for example, in Fig. 2, GZ is much greater than it is in Fig. 1, at 15 degrees. In Fig 3 these lengths are set up as ordinates of a curve, and similar lengths for inclinations of 45 and of 60 degrees are similarly set up. The curve drawn through their upper extremities is this vessel's "curve of stability," observing that the base line is divided into equal lengths for equal angle-intervals on any convenient scale.

As regards the "metacentre," I must explain that in former times, when "initial stability" alone was calculated, the word "metacentre" had a much more limited meaning than it possesses now. It formerly had relation to the upright position of the vessel, in which case the buoyancy acts upwards through the centre line of the ship, of course—along GM , for example, in Fig. 1. After receiving a slight inclination the vessel has, as we have seen, a new centre of buoyancy, and the buoyancy itself will act upwards along a fresh line, slightly inclined to what was previously the upright line, and will intersect it at some point M . This point was called the "metacentre," and if we suppose the angle in Fig. 1 to be very small (much less than 15 degrees), then the M shown there correctly marks the "metacentre." It is obvious that when a ship is much more inclined, the point at which two consecutive lines of the buoyancy's upward action will intersect may not be, and often will not be, in the middle line of the ship at all; but this point is nevertheless called the "metacentre," and the use of the word in this extended sense has recently become general. In Fig. 4 is shown a floating body of square section inclined in the water at an angle of about 30 degrees. WL is its water line, or line of flotation; B is its centre of buoyancy. By giving it a *slight* inclination from this position, it will of course have a new centre of buoyancy given to it. If we incline it one way b will show this; if we incline it the other way b' will show it; and for each of these positions there will be a new line of action of the buoyancy. But these lines of action, together with that through B , will all meet or intersect in one point, and this point M will be the metacentre at 30 degrees of inclination. In Fig. 5 I have represented the water lines, centres of buoyancy, lines of action of the buoyancy, and metacentres for a large number of angles of inclination, and in order to illustrate the change of position which the metacentre undergoes even in the case of so simple a form as a prismatic body of square section partly immersed, I have completed the metacentric curve, or curve of loci of the metacentres, for all angles of inclination, through 360 degrees, at intervals of 10 degrees. It may be said in passing that this figure is, of itself, sufficient to illustrate the very great and complicated changes which the stability of even this simple form of ship would undergo with varying positions of the centre of gravity.

In Fig. 6 I have shown curves of stability for this prismatic body with the centre of gravity in the centre of form, and also with that centre in some cases raised above, and in others placed below, the centre of form. In this figure the draught of water is taken at $\frac{3}{5}$ ths of the total depth of the prism.

In Fig 7 are shown similar curves of stability for the same figure, and under the same conditions, excepting only the draught of water, which in this is $\frac{1}{2}$ th of the total depth.

In Fig. 8 are shown similar curves under like conditions, but with a draught of $\frac{3}{8}$ ths the depth.

In Fig. 9 I have selected from the above curves those in which the centre of gravity and the centre of form have been taken as coincident, but at their respective draughts of water.

In Figs. 10 and 11 I have again taken cases in which the draughts of water differ as before, but in which the centre of gravity is, in the one set of cases (Fig. 10), taken at 1 foot and 2 feet respectively above the centre of form, and in the other cases (Fig. 11) like distances below it.

These figures serve to illustrate very clearly the error involved in the assumption that with stability at the upright position, and stability at 90 degrees,—or but little instability at the latter, which is what some authors have instructed the profession to be content with,—there need be no apprehension of any deficiency of stability at intermediate angles of inclination. They show that with square sections and prismatic forms there may be various dispositions of centre of gravity and draughts of water with which stability in the upright position, and again at 90 degrees, are not proofs of safety, but indications of the gravest danger.

The relation in which actual ships may stand to such cases as I have, for simplicity's sake, given above, may be readily seen by now proceeding to consider the curve of the "Hammonia." I am not in possession of the drawings of this ship, nor of the distribution of her weights, and have therefore not been able to calculate her curve of stability; but I have no doubt that the curve handed to me in evidence by Mr. Biles is a correct one, and it is reproduced on a smaller scale in Fig. 12. It will be seen that it very closely resembles the curve of stability of the prismatic ship immersed one-fifth of her total depth, and having the centre of gravity at the centre of form (slightly below the metacentre). However unexpected, therefore, may have been the state of the "Hammonia's" stability, it clearly lies within the view and grasp of science now that attention has been drawn to the subject. At the same time, I do not think that either Mr. Elgar or Mr. Biles in any way exaggerated the risks which must have been run at launches since the introduction of modern vessels with small metacentric height at launching draughts. The "Hammonia," instead of rapidly acquiring stability as she inclined, as it was thought and taught that high-sided ships would certainly do, in point of fact gained stability so slowly that although she had some substantial metacentric height ($6\frac{7}{8}$ inches) at 20 degrees, she had a righting lever of only $3\frac{1}{4}$ inches; at 30 degrees it was only $4\frac{9}{16}$ inches; beyond this it never much increased; at 50 degrees it was but $1\frac{1}{4}$ inches in length, and at 53 degrees it disappeared altogether.

It will be seen from the "Hammonia's" curve that after finally capsizing at 53 degrees, and passing through a phase of negative instability from 53 to 90 degrees, she then began to acquire positive stability again. In the "Daphne's" case this feature does not hold, even supposing her main deck water-tight; but in all the early stages of the curve, shown in Fig. 13, there is a very remarkable resemblance. In both vessels the initial stability was small; in both it grew but slowly as they inclined; in both the maximum stability lever was limited to about 5 inches; in both, this maximum (providing the vessel was water-tight up to the main deck only) was reached between 30 and 40 degrees of inclination; in both it then rapidly declined; and in both it wholly disappeared at little more than 50 degrees. In Part I. of this report I have assumed, in dealing with the actual capsize, that the side above the main deck may have contributed to stability during the brief period of the vessel's roll to port; and in Fig. 13 I have shown the curve of stability which would have held good until the gunwale became immersed, provided the side was perfectly water-tight up to the top deck. I have also shown the modification the curves undergo on the assumptions of the poop being water-tight, and of this alone above the main deck being so. It does not appear to me at all necessary to discuss at length the actual conditions or value of these upper structures in the present case, because, as I showed in Part I., there was, even on the most favourable suppositions, insufficient stability to keep the ship safe under the circumstances. It was at the early angles of inclination that the stability failed to come into play, as the curve clearly shows; up to 31 degrees neither poop nor upper works could assist; at that angle, with so much shifting weight on board, and such very small growth of stability, she must have acquired considerable momentum, and have accumulated considerable work. The stability remaining (even assuming the upper works to have been fully effective) was very little more than sufficient to absorb this "work," and could only have done so when she had reached the angle of about 50 degrees, where she was apparently held for a moment or two, and probably carried slightly back towards the upright. The vessel was in no condition, however, to withstand such leakages through her cargo ports, &c., and such further movements of weight as must then have taken place, and the completion of the capsize was inevitable.



In order to further illustrate the relative stability of both the "Daphne" and the "Hammonia," I have compared them with the curves of stability of two actual ships, of small initial stability, which have been carefully calculated, and which I will call ship A and ship B. Ship A has a positive metacentric height of $\frac{8}{10}$ ths of a foot, but ship B has a *negative* metacentric height of $\frac{5}{10}$ ths of a foot. In the latter case the vessel would loll over to about an angle of 20 degrees before she began to acquire positive stability.

I have shown these curves on Fig. 14, from which it will be seen that both the "Daphne" and "Hammonia," are at launching draught greatly deficient in stability, alike in amount and in range. The slowness with which they acquired additional stability is also remarkable as compared with the growth of stability in vessels A and B. I may add that "ship A" is a spar-decked vessel, 245 feet long, 32 feet 3 inches broad, and 24 feet deep, with a freeboard of 8 feet to the spar deck, and with full cargo of coals or grain. Her curve is extracted from a valuable paper, read before the Institution of Naval Architects by Mr. Benjamin Martell, entitled "On causes of Unseaworthiness in Merchant Steamers." "Ship B" I have taken from Mr. W. H. White's "Manual of Naval Architecture," published in 1882, and her particulars are as follows:—Passenger steamer, with miscellaneous cargo, laden, so as to be initially unstable, 312 feet 6 inches long, 33 feet 4 inches broad, and 16 feet 3 inches mean draught, and with about 9 feet 8 inches freeboard.

On inspecting the curves in Fig. 14, it will be seen that in the case of ship A, although her metacentric height is only $\frac{8}{10}$ ths of a foot, and the early growth of stability is not rapid, it increases quickly and largely after an inclination of 20 degrees is reached, and has scarcely reached its maximum at an angle (between 50 and 55 degrees), at which the stability of both the "Daphne" and "Hammonia" have wholly disappeared. In the case of ship B, which really does not greatly differ in natural stability from ship A, there is no positive stability up to 20 degrees of inclination, owing to the metacentre being six inches below the centre of gravity in the upright position. At 20 degrees a position of stable equilibrium is reached, and then the positive stability increases pretty rapidly, up to an inclination of about 45 degrees, when it attains its maximum. At the angle at which the stability of the "Daphne" and "Hammonia" disappear, it has not greatly fallen, and keeps positive to nearly 80 degrees of inclination.

In order to better exhibit the relative stabilities of ships A and B, with those of the "Hammonia" and "Daphne," at launching draught, and to illustrate the resemblance of these two vessels A and B as regards the amount of their natural stability, I have thought it well to eliminate the initial metacentric height, by assuming that they are so stowed as to bring the centre of gravity and metacentre together in each case respectively. For the sake of extending the comparison between these ships and the "Daphne" and "Hammonia," I have also modified their curves of stability to suit the condition of coincident centres of gravity and metacentres.

Fig. 15 accordingly shows these four curves of stability, with the metacentric height reduced to zero. This figure well illustrates the comparative lowness and shortness of the stability curves of the "Daphne" and "Hammonia."

Fig. 16 shows the curves of displacement, centres of buoyancy, and metacentres of the "Daphne" respectively, from which can readily be ascertained the positions of the metacentre and centre of buoyancy of the vessel at any given displacement or line of flotation.

It seems obvious that this "Daphne" disaster should be followed by a very general extension of the practice of fully calculating the stability of ships, and more especially of the practice of calculating "curves of stability" for vessels at successive draughts of water, from light or launching draught, to the deepest load draught, and for the various conditions of coal and cargo stowage. It may tend greatly to facilitate this desirable object if I state in conclusion that the labour and time of calculation are greatly reduced, with no loss of accuracy, by the employment of a very ingenious instrument known as "Amsler's Integrator," which is coming into use on the Clyde and elsewhere. Mr. C. W. Merrifield, F.R.S., fully described this instrument in a paper read by him at the Institution of Naval Architects in March 1880. He said, "The object of this instrument is to find the area, the statical moment, and the moment of inertia of any closed curve, by simply tracing out the curve with a pointer, the machine doing all the rest, and the results being read off directly from it without any trouble of calculation." He then proceeded to describe the method of working the instrument in detail; and in the discussion that followed, it was stated by a naval architect of eminence, "that any one who is familiar with ship calculations must know that this is a thing which we have been longing for, for years, because it will save us an immense amount of mere

“ routine work ; and if this instrument can be bought for a few pounds, it can be used
“ not merely for ordinary calculation of a ship's displacement, the position of the
“ metacentre, and the centre of buoyancy, but for the calculation of curves of displace-
“ ment, and curves of stability.”

In a paper read at the same Institution in March 1882 by Mr. J. Biles, entitled
“ Curves of Stability of some Mail Steamers,” the author gives a clear description of the
method of deducing a curve of stability from the drawings of an actual vessel by means
of this instrument. He says that the advantages of the method are—“1st, *greater*
“ *accuracy*, on account of the fact that the integrator calculates a discontinuous curve as
“ accurately as a continuous one, and also that the work is mostly mechanical, and
“ therefore not so liable to errors ; 2nd, *greater simplicity*, for there is much less work
“ involved, and it may be done by comparatively unskilled labour.”

I have the honour to be,

Sir,

Your obedient Servant,

E. J. REED.

FIG. 1.

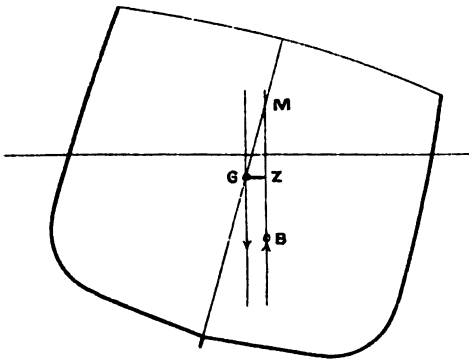


FIG. 2.

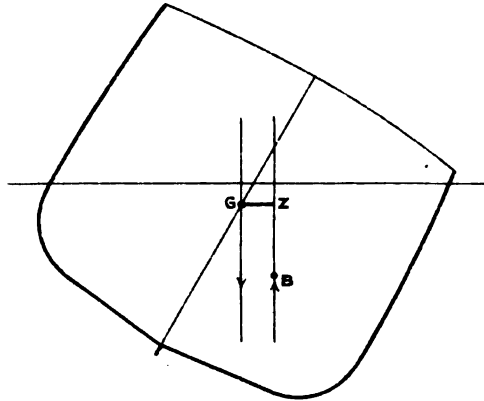


FIG. 3.

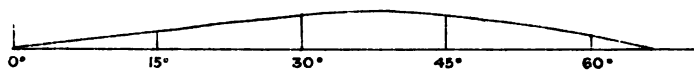


FIG. 4.

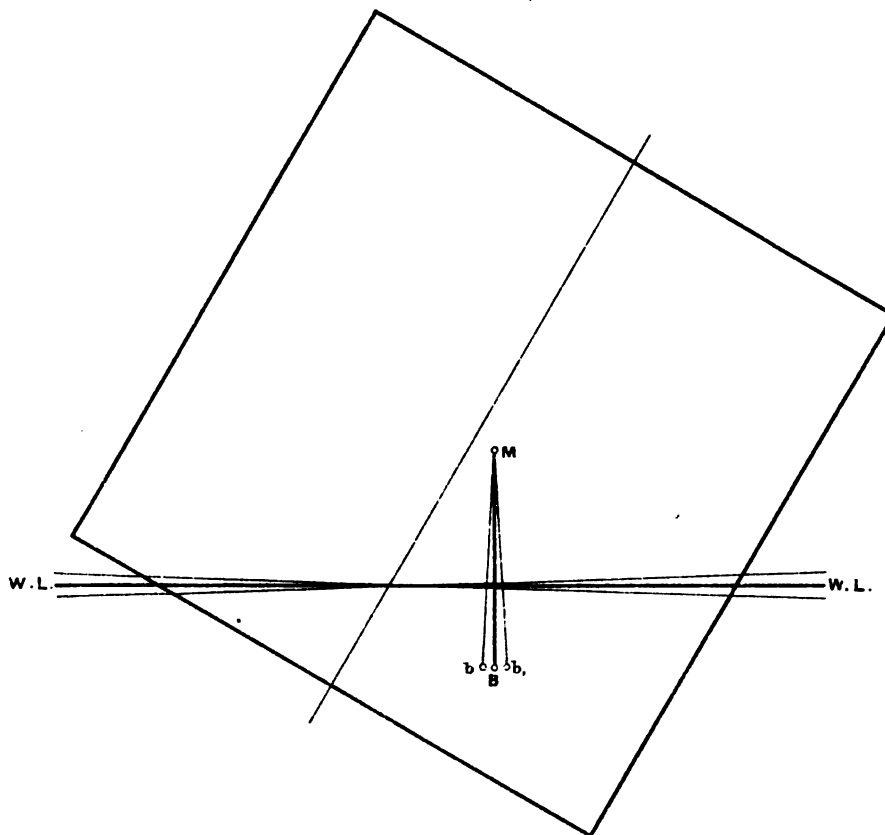


FIG. 5.

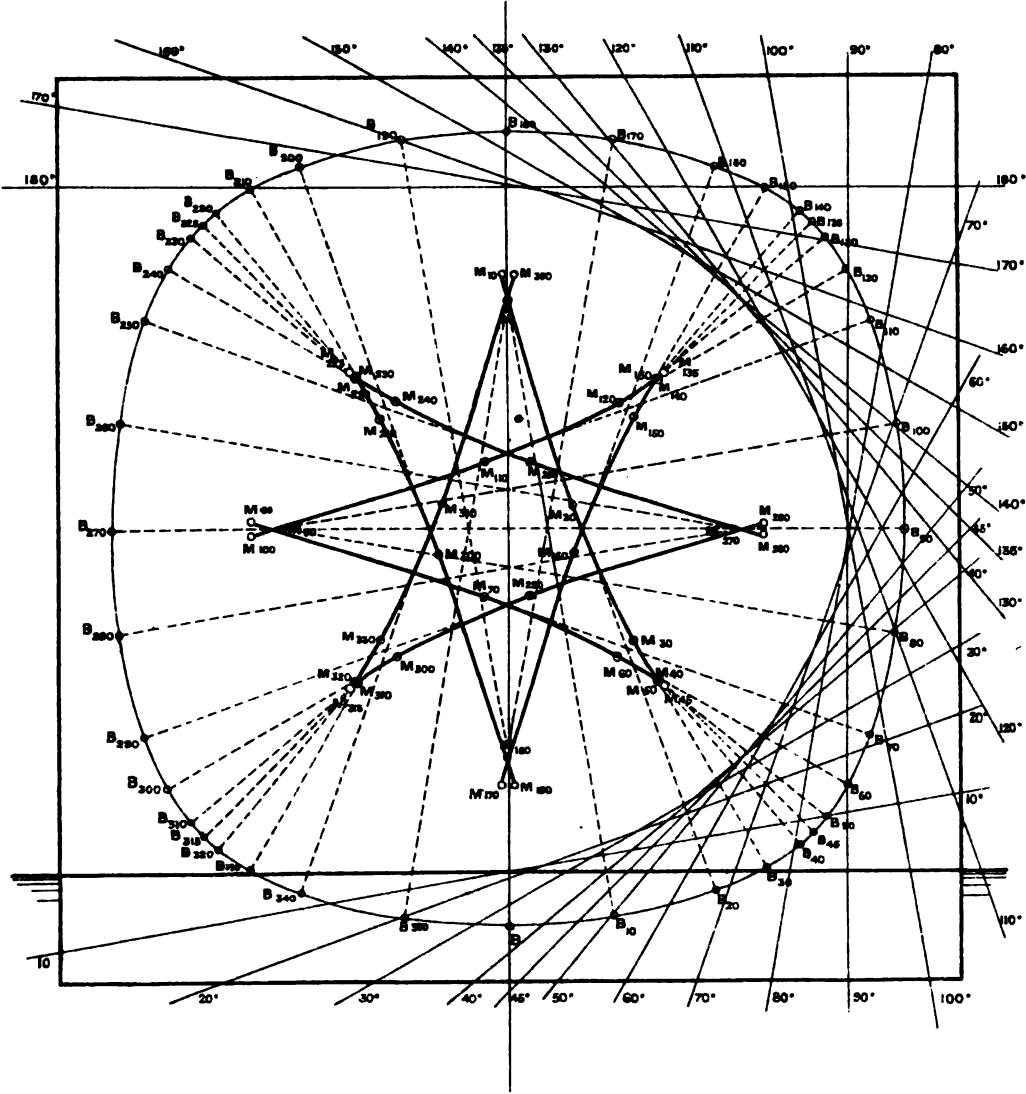
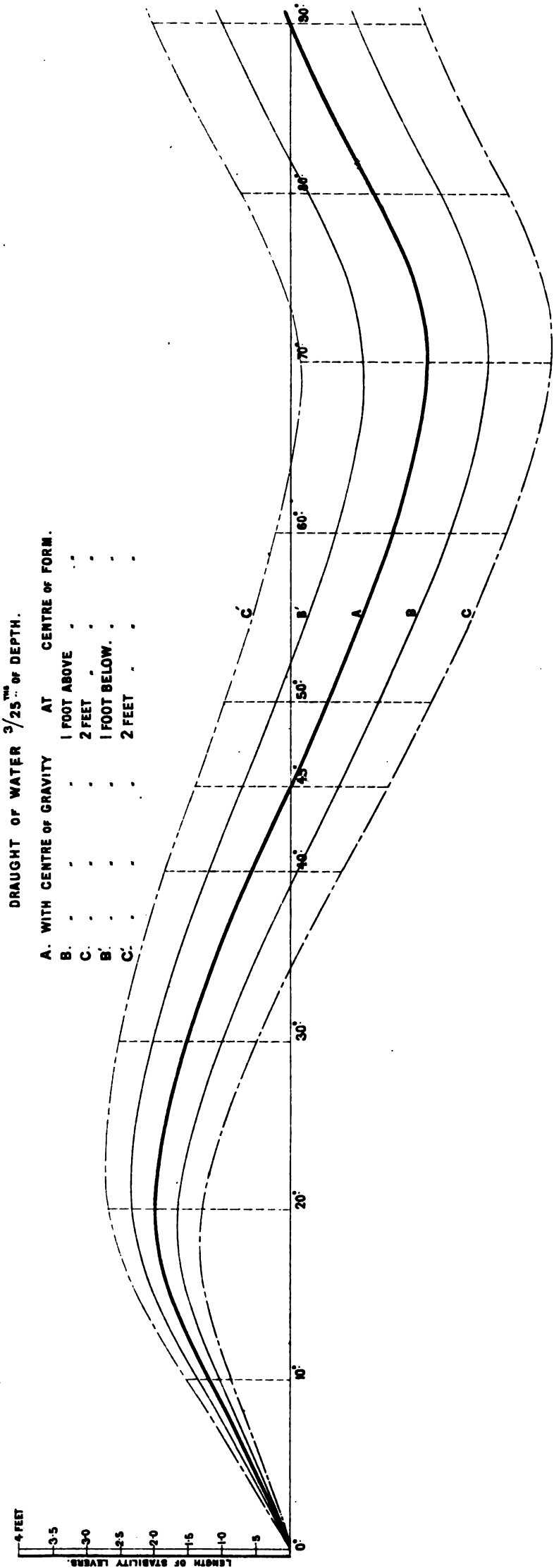


FIG. 6.
CURVES OF STABILITY OF PRISMATIC BODY OF SQUARE SECTION.



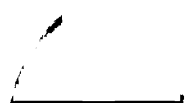
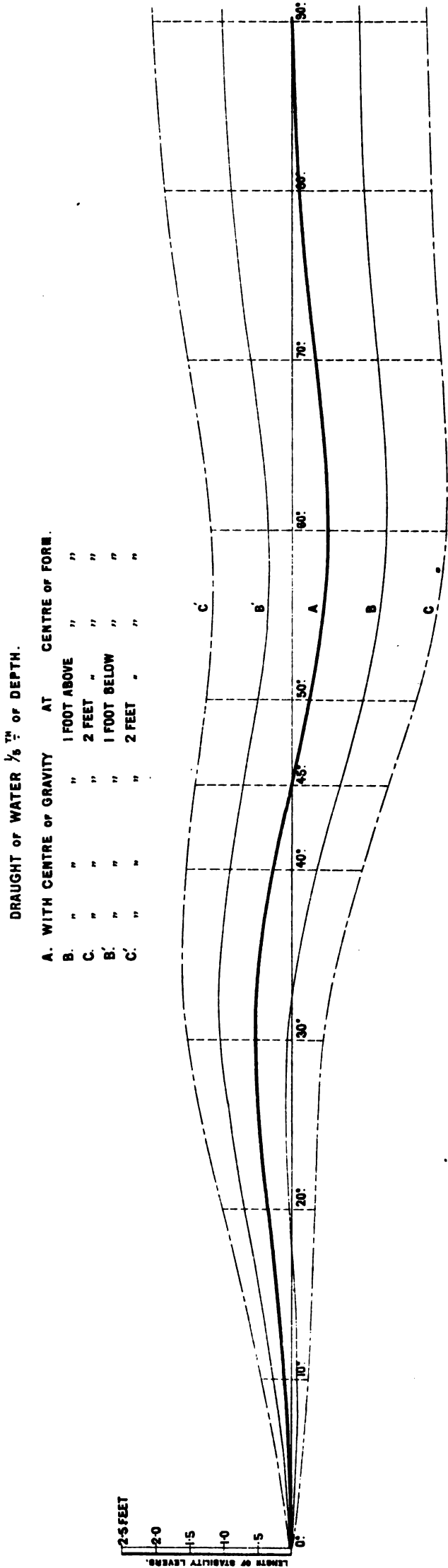


FIG. 7.
CURVES OF STABILITY OF PRISMATIC BODY OF SQUARE SECTION.



CURVES OF STABILITY OF PRISMATIC BODY OF SQUARE SECTION .

DRAUGHT OF WATER $\frac{2}{6}$ THS OF DEPTH

A. WITH CENTRE OF GRAVITY	AT	CENTRE OF EORM.
B.	"	"
C.	"	"
B'.	"	"
C'.	"	"

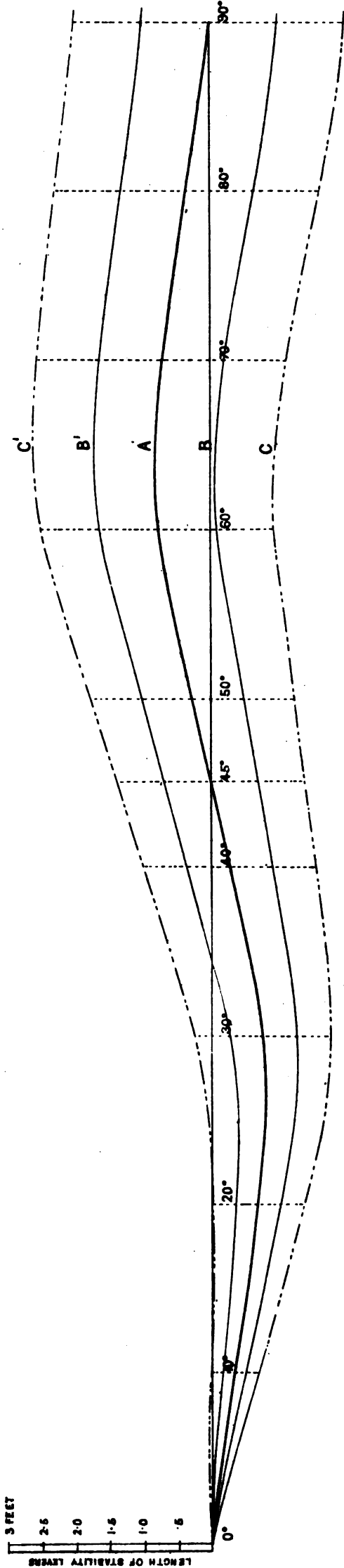


FIG 9.

CURVES OF STABILITY OF PRISMATIC BODY OF SQUARE SECTION

WITH CENTRE OF GRAVITY AT CENTRE OF FORM

A. WITH DRAUGHT OF WATER $\frac{3}{25}$ OF DEPTH		
B.	"	"
C.	"	"

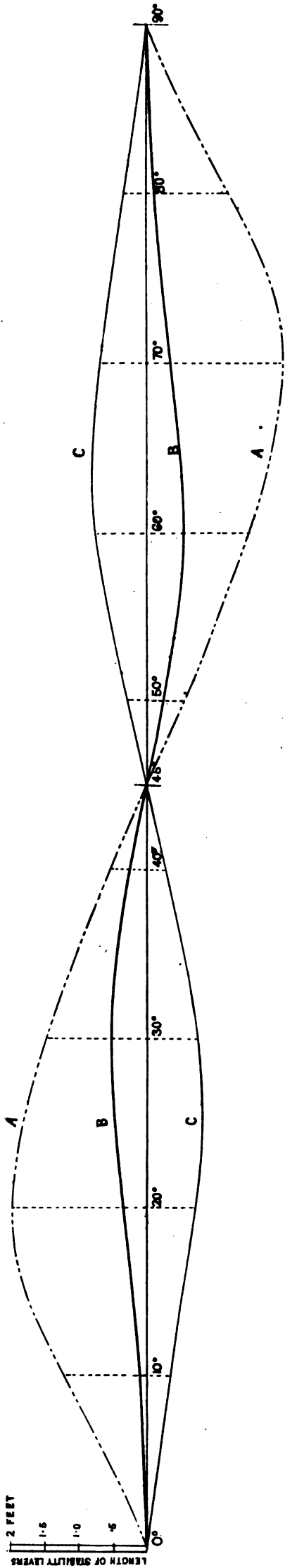


FIG 10.

CURVES OF STABILITY OF PRISMATIC BODY OF SQUARE SECTION.

DRAUGHT OF WATER $\frac{3}{25}$ THS OF DEPTH	{		A WITH CENTRE OF GRAVITY	{		1 FOOT ABOVE CENTRE OF FORM
			A'			2 FEET "
DRAUGHT OF WATER $\frac{1}{5}$ TH OF DEPTH	{		B WITH CENTRE OF GRAVITY	{		1 FOOT ABOVE CENTRE OF FORM
			B'			2 FEET "
DRAUGHT OF WATER $\frac{2}{5}$ THS OF DEPTH	{		C WITH CENTRE OF GRAVITY	{		1 FOOT ABOVE CENTRE OF FORM
			C'			2 FEET "

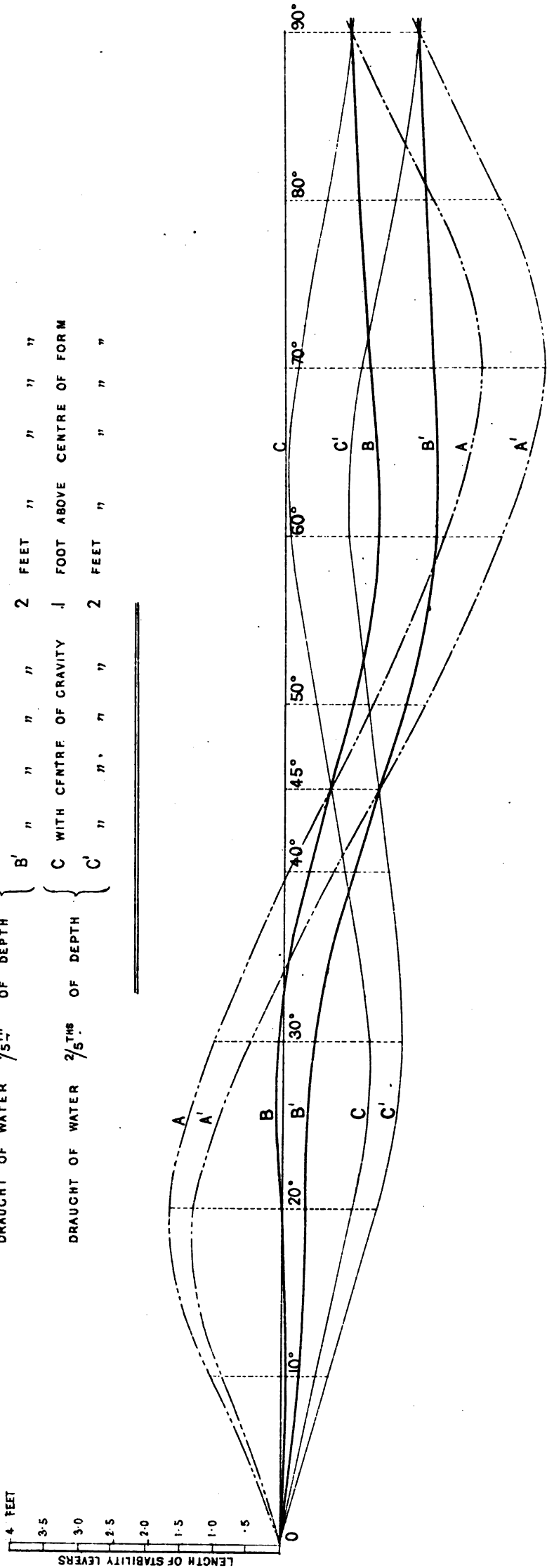
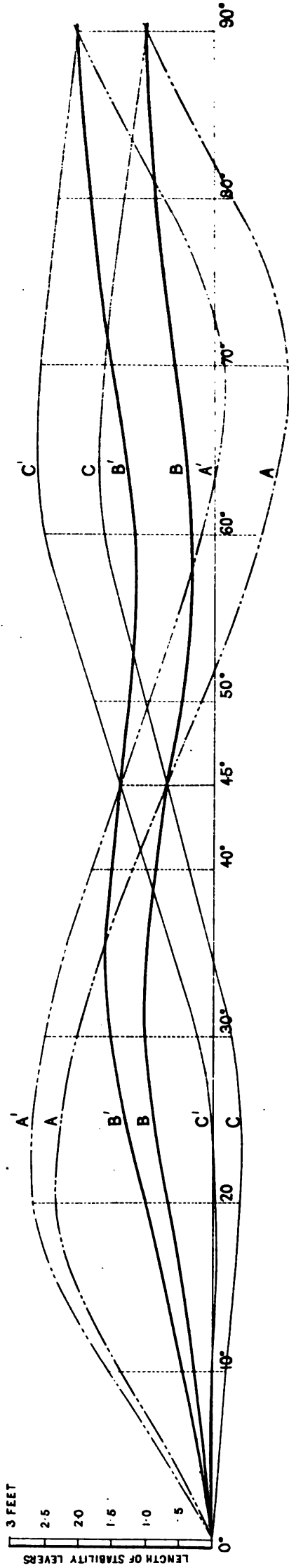


FIG II.

CURVES OF STABILITY OF PRISMATIC BODY OF SQUARE SECTION.



DRAUGHT OF WATER $\frac{3}{25}$ OF DEPTH	{		{		{	
	A.	WITH CENTRE OF GRAVITY	1	FOOT BELOW CENTRE OF FORM	A.	WITH CENTRE OF GRAVITY
	A'	"	"	2 FEET	"	"
DRAUGHT OF WATER $\frac{1}{5}$ OF DEPTH	{		{		{	
	B	WITH CENTRE OF GRAVITY	1	FOOT BELOW CENTRE OF FORM	B	WITH CENTRE OF GRAVITY
	B'	"	"	2 FEET	"	"
DRAUGHT OF WATER $\frac{2}{5}$ OF DEPTH	{		{		{	
	C	WITH CENTRE OF GRAVITY	1	FOOT BELOW CENTRE OF FORM	C	WITH CENTRE OF GRAVITY
	C'	"	"	2 FEET	"	"

CURVE OF STABILITY OF S.S. "HAMMONIA" AS LAUNCHED.

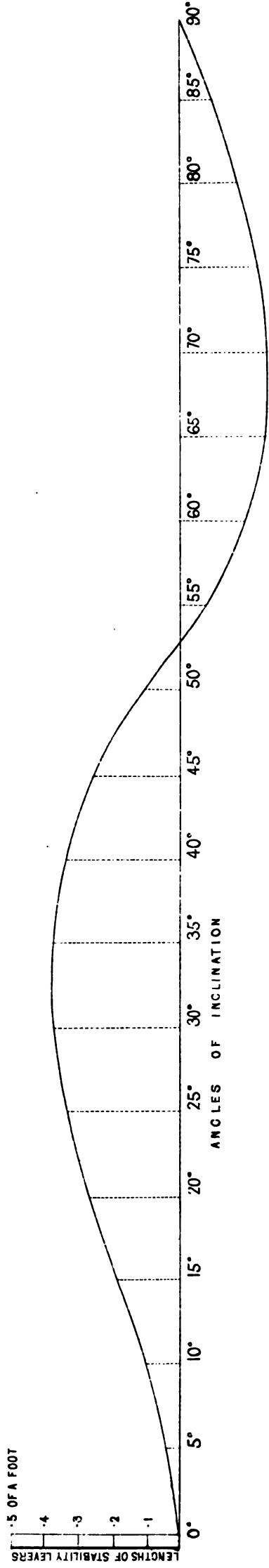


FIG 13.
CURVES OF STABILITY OF S.S. "DAPHNE"
AS LAUNCHED.

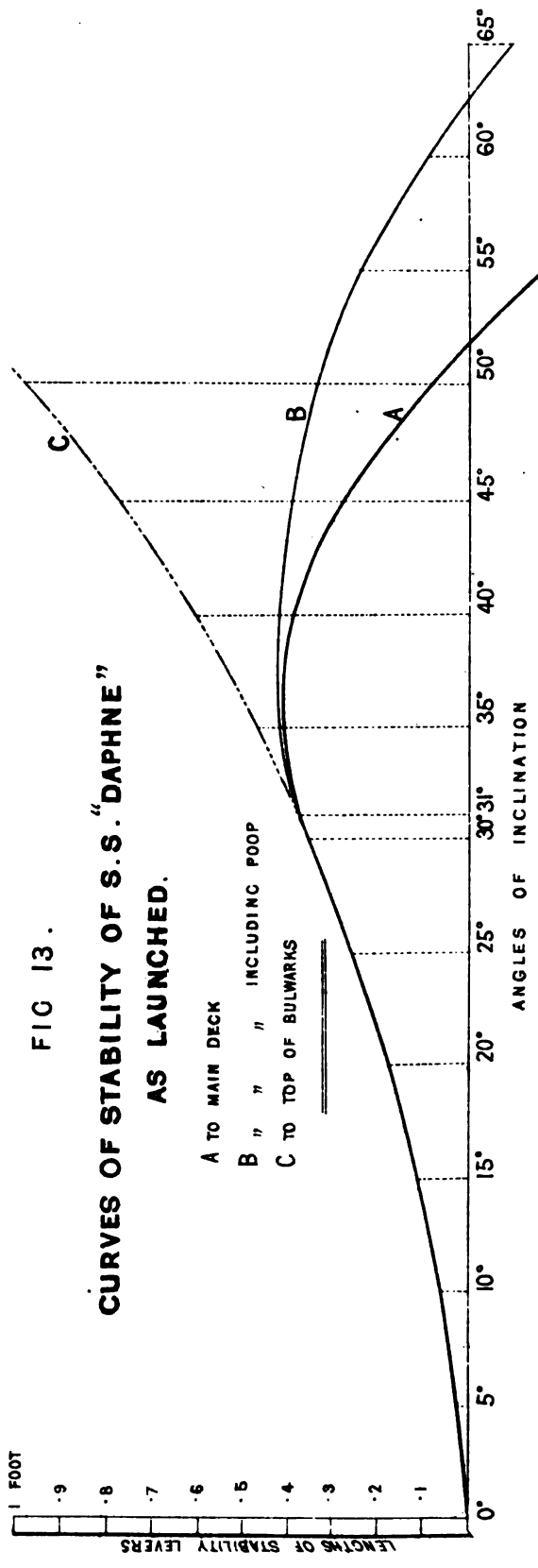


FIG 14.

CURVES OF STABILITY

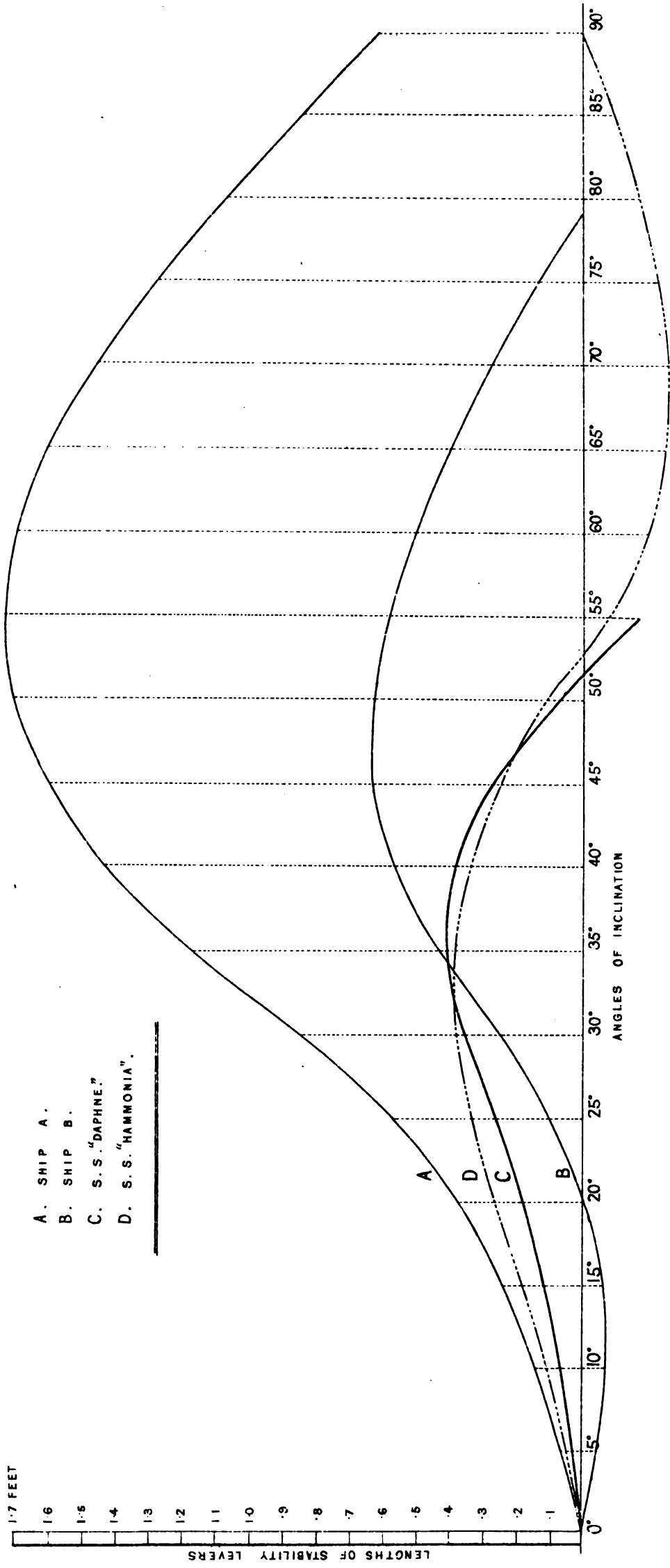


FIG. 15.

CURVES OF STABILITY

METACENTRIC HEIGHTS REDUCED TO ZERO.

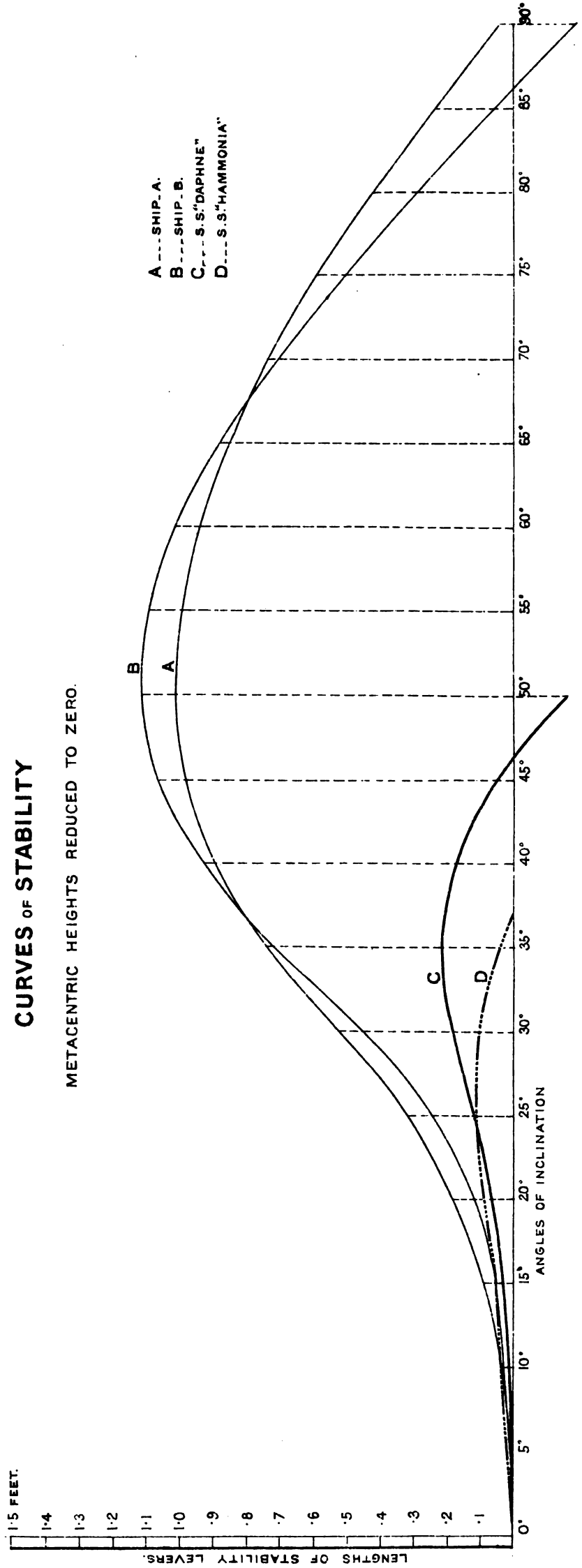
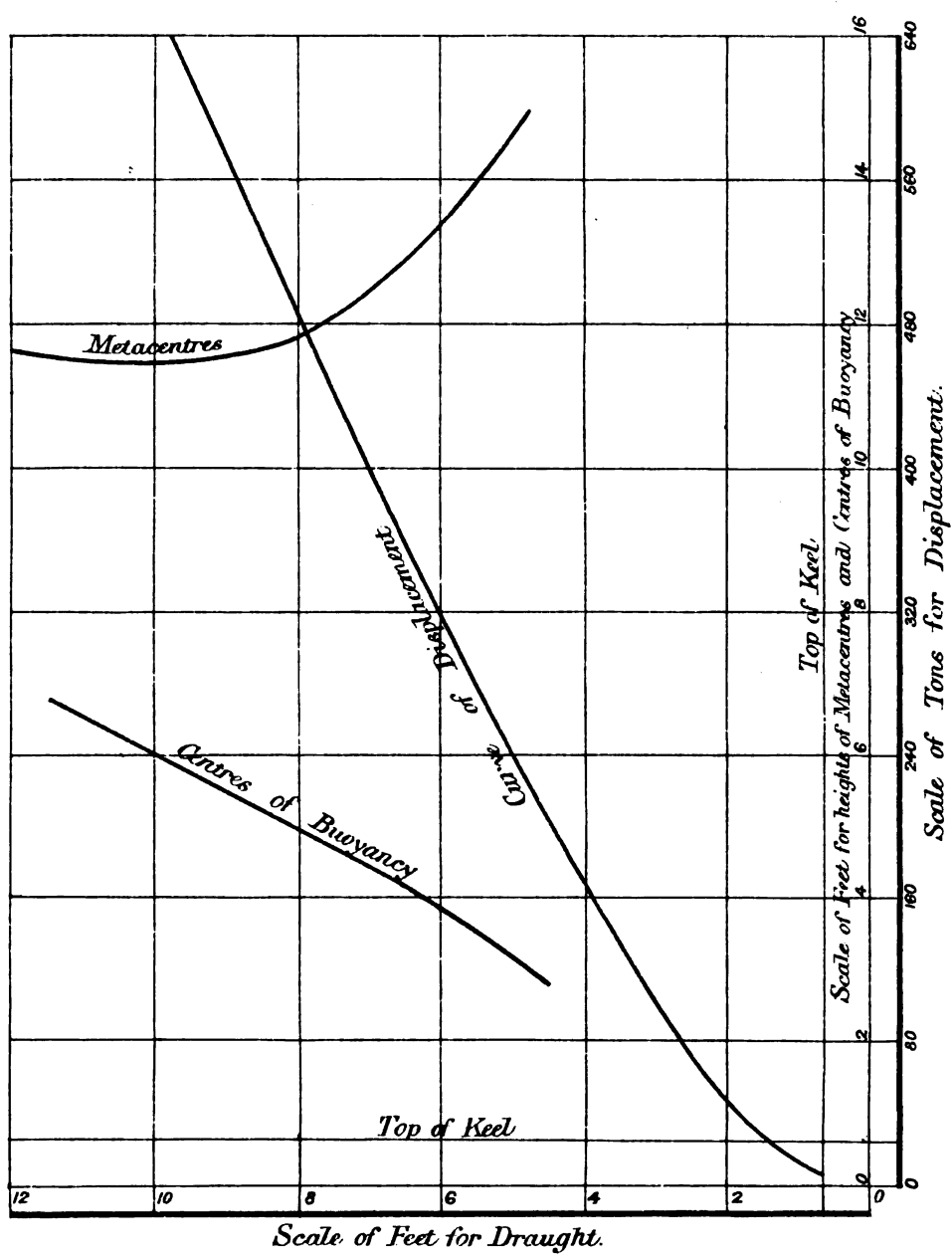


FIG. 16.

S.S. "DAPHNE"





THE "DAPHNE" INQUIRY.

NOTES OF PROCEEDINGS AT INQUIRY INTO THE SINKING OF S.S. "DAPHNE" AT GLASGOW.

BEFORE SIR EDWARD J. REED, K.C.B., F.R.S., M.P., the Commissioner appointed to make the Inquiry
MR. R. P. LAMOND, WRITER, GLASGOW, *Assessor*.

PROFESSOR ROBERTON appeared along with the Partners of the Builders' Firm, Messrs. Stephen & Sons.

Glasgow, Tuesday, July 10th, 1883.

The COMMISSIONER said: It is proper that I should state at the commencement of these proceedings that their object is to inquire into the cause of the recent deplorable accident at the launch of the vessel "Daphne" in the river Clyde. Therefore, whatever evidence may tend to throw light upon the actual cause of the accident I will be most ready to receive.

At the same time it must be clearly understood that the scope of this inquiry will be strictly limited to the object which I have stated, namely, the cause or causes which produced the capsizing of the ship; and that this inquiry will not supersede or interfere with the action of the Crown Authorities in Scotland.

In the case of a disaster of this kind, so awful in its consequences, many contingent questions may be expected, indeed, are certain to arise; but with these I have no authority whatever to deal, nor is it any part of the object of this inquiry to deal with any collateral considerations or consequences whatever. I make this statement because it is most desirable in the public interest and in the interests of all concerned, that no misapprehension as to the objects and scope of the inquiry should arise.

It may be convenient for me to indicate in outline the principal points upon which information is desirable. These are—

The manner in which the ground ways, bilge ways, and cradle of the launch were arranged and constructed.

The length and declivity of the ground ways.

The height of tide at the time of the launch, and the velocity of the current; the position of the ship

at the time when she began to careen—whether she was wholly or only partly off the slipway.

The nature, number, and arrangement of the drag anchors, chains, and plates that were employed to check the motion of the ship when launched.

The character of the ship itself; its intended draught of water at the time of launching; its stability at that draught.

The weights and position of the engine how far they were firmly secured or otherwise.

What other weights were on board; where they were situated; whether they were loose or made fast in place; the number of persons on board at the time of the launch; in what parts of the ship they were employed; where they were at the moment of the launch.

Besides the above, with other requisite particulars which it is not necessary here to enumerate, the fullest information is desired respecting any observations of the accident that persons on board of the "Daphne" at the time or witnessing the launch from the shore may be able to put in evidence.

Evidence will be taken upon oath.

Professor ROBERTON said: In attending, as I do, on behalf of Messrs. Stephen & Sons, the builders of the "Daphne," I beg to mention that the partners of that firm are in attendance in response to the intimation which you have addressed to the public, and, if wished by you, Mr. Stephen, the senior partner of the firm, will at once proceed to answer such questions as may be put to him.

Mr. ALEXANDER STEPHEN sworn and examined.

(*The Commissioner.*) I shall be very glad to hear from Mr. Stephen any statement he may favour me with with reference to the launching of the vessel.

(*Professor Robertson.*) For the purpose of saving time Mr. Stephen has written out a little memorandum which, perhaps, you will allow him to read.

2. (*The Commissioner.*) Certainly. The witness then proceeded to state:—I am senior partner of the firm of Alexander Stephen and Sons. The firm commenced business in Glasgow about 33 years ago, and

up till now we have built about 280 ships; the gross tonnage of these is 290,000 tons. At present we are building for owners in Scotland, England, Italy, Germany, and France. In 1881 we built 21,860 tons of shipping, gross tonnage, and last year we built 21,470 tons. Since 1871 we have made all our own engines and boilers. The steamer "Daphne" is intended for the Irish trade, and has been built for the Glasgow and Londonderry Steam Packet Company entirely to their specifications and plans. The

*The
Commissioner.*
10 July 1883.

*Mr.
A. Stephen.*

Mr.
A. Stephen.
6 July 1883.

"Daphne" is an iron screw steamer of about 460 tons gross register, and about 200 tons nett register. Her length between perpendiculars is 176 feet 3 inches, her breadth extreme is 25 feet 2 inches, and her depth of hold 13 feet 3½ inches. She has a full poop, topgallant forecastle, and a bridge deck. The engines are of the usual compound surface condensing type, and about 120 horse power nominal. The arrangement for the launching was made in our usual way, as in the case of all our previous 280 ships, which were without accident, not so much as a single man ever being hurt. I was not myself present at the launch, happening to require to be present in the capacity of Dean of Guild for the city of Glasgow at an important meeting in the Merchant's House. In consequence of this I cannot describe the launch itself, but I arrived at the works about half-an-hour after the accident. I found that the most prompt and energetic measures were being employed for the treatment of the men who had been in the water, and since then incessant efforts have been made in the direction of recovering the bodies of those who were drowned, and preparations for the ultimate raising of the ship. I had not the least thought of any danger in connection with the launching, and but for the mere circumstance of the not having got finished with some work in another vessel then in the harbour my eldest son and the son of my partner, Mr. Kemp, would certainly have been on board the "Daphne." I should mention that it was no saving of money to the firm, but the reverse, to press on the work that was in her in the way of calling for so many men to be on board. Should you, Sir Edward, wish to put

any questions, I need not say that I shall be happy to answer them in so far as in my power.

3. I understand you to say, Mr. Stephen, that the "Daphne" was built to the plans of the owners of the vessel—the company for which she was built?—Yes.

4. You heard my general statement just now with regard to the scope of this inquiry?—Yes.

5. Do you yourself wish to give further evidence upon any of the points I there mentioned, or upon any similar points, or do you propose to suggest other witnesses as representing your firm?—There will be others who will speak to any details you may desire with regard to the ship or the launch.

6. Would you mention who these witnesses are?—We will require to take time to consider who they are to be. There are several gentlemen in attendance at present who could give such information as you may desire, but we have not determined on the names of the gentlemen who will be called.

(*The Commissioner.*) I should like to take evidence first upon the launching arrangements, and upon the position of the vessel at the time when she began to capsize, because, if it should appear that the ship was fully afloat at the time, it would be unnecessary to enlarge the evidence upon the nature of the launching ways and cradles, and so forth. Therefore, I should like at once to take a witness or witnesses upon the construction of the launching ways.

(*Professor Robertson.*) In that case, Mr. John Stephen, the second partner of the firm, who is in attendance, will now give evidence if wished.

Mr.
J. Stephen.

Mr. JOHN STEPHEN sworn and examined.

7. (*The Commissioner.*) Do you propose to tender any evidence, or only to reply to questions?—If you will allow me, I will read a statement which I have prepared. The witness then read the following statement:—

I am one of the partners of the firm. I have heard my brother's account of the origin and history of our firm, and I concur therein. I have been a partner since about 1860. The practical work of the yard is more under my supervision than that of my brother. I was the only one of the three partners present at the launch of the "Daphne," my brother being absent on official duties, and Mr. Kemp on some other work. The launch was under my direction, assisted by our manager, Mr. McMaster, and head foreman carpenter, Colin Wallace, and other foremen. The launch took place about 11.25 a.m. on Tuesday, 3rd July, about half-an-hour before high water at Glasgow, and 10 minutes before high water at Linthouse. It takes about 20 minutes for the water to travel from the Broomielaw Bridge—where our time tables take it from—to Linthouse. At the time of launch the engines, propeller, shafting, and other machinery, except the main boiler, were fitted on board and finally bolted and secured in their respective positions. The boiler hatchway or hold was open, and decks removed on starboard side to admit of the main boiler being put aboard immediately on the vessel's arrival at the crane at Broomielaw; the loose plates, planking, &c., being stowed handy on the decks. The general fittings and joiner work were in an advanced state. The necessary materials for finishing the work in the harbour being aboard the vessel and disposed on the various decks and holds. There would be about 200 men aboard. The position of these men would be mostly on the upper or spar deck, and in the middle or main deck, at the various work there. At the time of a launch, you may be aware, that men generally leave their work and possibly get on the upper deck. I cannot exactly speak to that occurring here, but it is usually the case.

8. Where were you at the time of the launch?—I was standing on the gangway at the right head of the ship, on the christening gangway, on the stage immediately in front of the ship.

9. You stated that the vessel would have proceeded but for the accident to the crane at Broomielaw?—Yes.

10. Would you state where she would have been finished; how long she would have been there; and where she would have been taken after getting her boilers in?—She would have remained at the Broomielaw at the most convenient place that the Harbour-Master could give us for convenience to our works, and would have been finished there in the course of 10 days from the time she was launched. The launching ways were each 18 inches wide, and spaced 8 feet 3 inches apart, inside to inside, at shore end, and 8 feet 4 inches apart at water end, and had a declivity towards the river of ½ inch at bow of vessel, ⅙ inch at stern, and ¼ inch at outer end of ways; there being a camber or round on their entire length.

11. Will you, when convenient, put in a drawing or tracing of the ways?—I can give you one now. (*The witness produces a tracing.*)

12. Were these declivities obtained by making a curve of the ways, the arc of a great circle, or anything of that kind?—Yes, they have a regular circle. We always lay our ways in a round.

The vessel herself was built on a declivity of ⅙-inch per foot on the keel. The sliding ways were 125 feet in length, the fore end being 23 feet 6 inches from bow, and after end 27 feet six inches from after side of stern post. The up-making extended the whole of this length, and being well and carefully fitted, and held in position by chains and shores as usual. The ways were new, and made of oak and pine, with guide battens on inner edges of sliding ways. After the launch the standing ways remained perfectly intact. We have got prepared, and I now put in a sketch of the launch. We had it prepared in a great hurry, since yesterday, I think. The drags for checking the vessel were two in number, the one on starboard side being secured to a plate, bolted to side of vessel, above the belting. The centre of shackle eye being 12 feet 10 inches from centre line of vessel, and 71 feet 9 inches abaft foreside of stem, measuring parallel with centre line. The cable chain was of 1⅜-inch diameter, and having a drag of chain of about 17 tons weight, and from same cable chain a separate chain

was led past the drag chains nearer the bow, and secured to an anchor weighing about 12 cwts.

13. Was that anchor buried?—Half into the ground. This was followed up with a drag of plates weighing about 2 tons 6 cwts., with the chain attachments secured to the upper fluke to keep the holding fluke in the ground. On the port side there was a chain of $1\frac{3}{8}$ diameter led through the hawsepipe and carried over the windlass, then down into the chain locker and secured round the keelson. To the shore end of this chain an anchor weighing about 7 cwts. was secured, also followed by a drag of plates weighing 2 tons 5 cwts., including lashing chains, and secured to upper fluke to keep lower fluke well into the ground. The whole weight of the drags on both sides, apart from the holding power of the anchors, being about 22½ tons, exclusive of chain between drags and ship. It has been ascertained that the drag chain on the starboard side drew the pile of chain for a distance of 90 feet; the anchor behind same dragged 15 feet 7 inches. The chain starboard drag took the ship 33 feet 9 inches before the port drag anchor. The drag on port side was drawn for a distance of 56 feet 3 inches, the fluke of the anchor remaining well in the ground all the while. When the dog shores were knocked away the vessel started at once and went into the water in a steady even manner, the up-making never showed any signs of giving way till the vessel was clear of the standing ways.

14. Can you tell me how far the vessel was supposed to be allowed to advance before any of these chains began to act upon her?—I would say about 37 feet. She was clear of the ways 37 feet before the first chain tightened.

15. Clear of the ways?—Yes, clear of the ways. We always allow a little bit to get the way off the vessel a bit before we begin to drag.

16. Then there is no doubt in your mind that the vessel was wholly clear of the launching ways?—I am very clear upon that. That can be ascertained when the chains are got up. There was no indication of uneven pressure. The speed was not greater than is our usual custom. The vessel ran clear out from the end of the ways, and was checked first by the chain drag on the starboard side, then by the anchor drag on port side, as already described, and subsequently by all the drags bearing at same time. On the vessel leaving the ways she listed slightly over to port, but recovered herself completely when brought up—then inclined to port again—again recovering to upright, and a little over to starboard, and then hesitatingly went over entirely to port.

17. On that return to starboard, did she pass the upright position?—I think she passed the upright position; that is my impression.—The vessel drifted up the river about 85 feet at stern and 30 feet at bow before she settled down. During the day of accident and following day the vessel changed her position, the stern having been pulled some 30 feet across the river, and consequently nearer the shore. Since the launch the vessel has gradually settled down aft about 20 feet, thus making the difficulty of raising the vessel much greater, so much so that this morning we have decided to get the assistance of pontoons in these operations.

18. In all these particulars which you have given, and which are matters of observation, such as the oscillations of the vessel, are they the result of your own personal observation?—They are the result of my own observation. In moments of this sort, and with occurrences like this, I defy any one to give

what actually takes place, but that is my impression as it occurred to me of what happened.

19. I only want to know whether you have been giving me the result of your own observation?—I have asked others about it and they vary. Every one will tell a different story, but that is my impression of what occurred. Everything was done at the time that was within our reach or power to save life at the time of the accident, and since then I have given all my energies and strength in the direction of raising the vessel, and have been there night and day, except for sleep and other necessities, until I had to get up this paper and prepare for the evidence before this Court. I will be glad to give you any further information you may ask me for.

20. Can you give me any evidence as to the depth of water over the launching ways at the time?—We had 4 feet of water on the launching ways.

21. At what point?—4 feet on the outer end of the launching ways.

22. At the time of the launch?—Yes, that I am certain about. I looked it; and below the launching ways—I have not measured it—but I believe we had 4 feet: that is 8 feet of water over the end of the ways.

23. It would be almost essential to this inquiry that one should have the drawing of the ship. Would you be willing to put that in?—I have not got a complete drawing of the vessel. I have got the lines.

24. I should like the drawing and specification. If not now, perhaps at a later stage you will give it.—Yes, we will get that and send it in at once.

25. What can you state as to the exact correspondence between the ship as built and the drawings you will put in? Do you allow any considerable departures from the drawings in the construction of your vessels?—The drawings that we would put in would be as near the complete drawings of the vessel as it is possible to make them. We could show of course in these drawings a little more the opening on the vessel's starboard side for getting in the boilers.

26. You have stated that the engines of the ship and the shafting and propeller were on board. In the drawing, will you have shown the position of the engines?—Certainly.

27. With regard to your evidence as to the amount of the weights on board; can you supplement your evidence either now or at any later time with any further information on that question?—Perhaps at a later time I will be able to do it. Do you refer specially to the weight of the ship as she is, or to the weight of the loose things on board the ship?

28. Any information on the question. Of course, you know perfectly well that it would be impossible to judge of the condition of the ship without having a drawing to give the form, and the specifications to give the amount, disposition and size of the material, and consequently of the weights, and also the particulars of the weights on board. Can you state now the weight of the engines?—I think I will leave that for Mr. Kemp; he will give you the exact weights as far as he can.

29. The only other question I would ask you is, whether you desire to repeat the statement of Mr. Alexander Stephen, to the effect, that whatever further information may be desired for the purposes of this inquiry, you will be willing to furnish to the best of your ability?—I shall be very glad indeed to do that.

Mr. EBENEZER KEMP sworn and examined.

30. (*The Commissioner.*) You are a partner of the firm of Alexander Stephen & Sons?—I have been a partner since 1871.

31. Would you state what is the particular branch of the business, if any, which you supervise?—The engineering department more especially.

32. Have you any evidence to tender to me?—First, I may say that I concur generally with the

evidence that has been tendered by the Messrs. Stephen, and that I can speak more positively to the position and state of the machinery that was on board.

33. Will you favour me with any particulars of the machinery that you may have with you?—The parts of the machinery that were on board were those stated by Mr. John Stephen, that is, the propeller and the shafting and the engine complete, and the donkey boiler as well.

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Mr.
E. Kemp.

Mr.
E. Kemp.
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34. Where was the donkey boiler placed?—It was placed at the forward end of the stokehole, close down to the keel.

35. Was it fixed in position?—It was put in in a semi-circular recess, with about 6 inches of clearance on either side, and fixed in the usual way that donkey boilers are fixed, not so perfectly as the engines would be fixed for standing upsetting, but bolted down to riders.

36. Were the fastenings as complete as they were intended to be, or only partly complete?—They were quite complete.

37. Can you give me the weight of that donkey boiler?—About 5 tons.

38. What can you tell me about the weight and position of the engines?—The weight of the engines is about 60½ tons as at present in the ship. I should like to correct what I said about the weight with reference to the donkey boiler. It is not as I gave you, 5 tons; that is the weight with the water in it, but at present it is 4 tons.

39. Has any calculation been made of the height of the centre of gravity of that machinery that was on board?—That is all in course of preparation, and will be handed in complete. It is nearly ready, but not just ready. I may say that the engine itself, and the shafting, and all the appurtenances connected with it, are quite complete. The engine is thoroughly and completely bolted down; in fact, it is ready for steam almost, with the exception of the attachments to the boiler. The engine will be in its position without any possibility of shifting.

40. Do you suppose that the holding-down bolts of the engine would be such as to have kept the engine in place after the vessel capsized?—I think perfectly. I don't anticipate any movement of the engine whatever.

41. You were not present at the time of the launch?—No, I was on board of another vessel, and we were just finishing. I was there very shortly after the accident.

42. You will, when ready, place before me this further information about the engines, giving the weights and so on?—Yes.

43. Have you any information that you can give concerning the other weights on board the ship, from your own personal knowledge?—We are just getting all that up, and it will be handed in along with the other information.

44. Have you any other evidence that you wish to furnish me with?—I think not.

45. (*Professor Robertson.*) Should you not say something about the state of the launching ways?—I have already concurred in the statement of Mr. John

Stephen on that matter; but I can speak also to the efficiency of the launching ways that they were all thorough and complete, and after the launch, on examination, nothing had given way. They were all quite sound, and thoroughly made up in every way.

(*The Commissioner.*) I may say at once that unless the evidence of Mr. John Stephen as to the effectual launching of the ship and the fact of her being entirely clear of the ways is to be controverted in any way, it will be quite unnecessary to pursue the inquiry into the construction of the launching arrangements, because unless there is something that is not observable on the surface, I satisfied myself by inspection that there was no fault to be found in that respect, and that we must look for the causes of the accident apart from the launching ways and appliances, from which observation I exclude for the moment the question of the drag-chains. I think this may shorten the inquiry, for unless evidence is produced to the contrary, the inspection convinced me that the launch was unobjectionable in every way so far as the launching ways and bilge ways, and cradle, were concerned.

Professor Robertson said: Sir Edward, this inquiry has come so suddenly upon my friends, the Messrs. Stephen, that really they have not had time to make the necessary preparations for bringing up all their witnesses at once. I am afraid that, looking to the circumstances, I am under the necessity of asking you to continue the inquiry until to-morrow, at the same hour, when we will be prepared to go on with our witnesses in succession without any interruption. I am exceedingly sorry that I should put you to this inconvenience; but in the circumstances I think it is unavoidable.

(*The Commissioner.*) I may say at once, that owing to the suddenness with which the inquiry has been commenced—owing to very urgent and proper causes, in my opinion—all that could have been expected to be done by these gentlemen at this time has been done, and I am much obliged to them for having taken the trouble they have done so far. Before adjourning until to-morrow, however, I would now ask whether any other witness is ready to give evidence now? [No answer.] Of course, *Professor Robertson*, the same difficulty that Messrs. Stephen have experienced in the way of getting up evidence, may have been experienced by others.

(*Professor Robertson.*) Quite so. Perhaps, Sir Edward, you will allow me to add that every member of the firm of Messrs. Stephen & Sons will be happy to come back and answer any questions that may be put to him.

(*The Commissioner.*) Then, I adjourn till to-morrow morning at 11 o'clock.

Glasgow, Wednesday, 11th July, 1883.

(*Professor Robertson.*) You requested, Sir Edward, that the model and specification and the plans of the vessel should be put in. Mr. Stephen, the senior partner, is now in attendance for the purpose of doing that. I presume you don't require him to be put on oath again?

(*The Commissioner.*) No. I may just say that I don't think it quite fair to the firm to put the model in, nor is it necessary for the inquiry. The drawings are all we require.

(*Professor Robertson.*) But will you accept the model if we tender it?

(*The Commissioner.*) Certainly.

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A. Stephen.
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Mr. ALEXANDER STEPHEN, re-called.

According to your request, Sir Edward, I now hand to you a copy of the specification which is an exact duplicate of the original, also the plan of the general arrangements of the ship, the lines of the ship, the midship section with scantlings marked, and also the displacement scale, and the rigging plan. These, I think, embrace all that you require or asked of me.

46. (*The Commissioner.*) Would you explain which of these were contract drawings, or were there any drawings as forming part of the contract?—No.

47. May I assume that the manuscript alterations on the printed specification have been conformed to in the ship?—Just the same.

48. I think it would be desirable, in order to get on the notes the information which you have now given, if you would be good enough to take back these drawings, and describe them one by one, so as to have them recorded properly?—Very well. There is, first, the specification of the screw steamer "Daphne," which was built for the Glasgow and Londonderry

acket Company, giving the dimensions and
s of iron of the ship, the arrangement of the
ys and water ports, and all the wood work of
specified in detail. There is also the masting
hip, and the arrangements of the cabins; all
ills of that are given. Then there is the
casing, skylight, and the chart-room, fore-
de lights, scuppers, and the outfit and furnish-
the ship. Then the engines are described in
nd the boilers and the steam winches, and
7 all the furnishings of the engine-room.
ation put in and marked No. 4 of the inven-
roductions.)

think you spoke of water ports, did you not?

here are only three square ports or rectan-
rts shown on the drawings of the vessel, three
side. I suppose they are all water ports?—
e on each side.

hey are shown about the height of the upper
Yes.

nd I take it for granted that they are all
rts?—Yes. Then the plan before me (marked
inventory of productions) is pretty much a
out of the specification. It is really the
ion put in detail. The next plan (marked
the inventory of productions) is the midship
with all the scantlings, which are also taken
specification. The next plan (No. 7) shows
of the ship. No. 8 is the displacement scale.
the rigging plan. No. 10 is the model of the

ssor Robertson.) I purpose to call in the first
Sir Edward, the superintending engineer of
et Company, for which this vessel was

Commissioner.) Before that is done, I should
sk Mr. Stephen or yourself a question which
ou can answer. Can you give us any par-
f the weights, because this information which
hen has so freely put forward, though very
and indispensable to the inquiry, does not

give any particulars of the weights, although of course
it enables a calculation to be made?

(Professor Robertson.) I could give you that infor-
mation in the course of an hour or so. It is all but
ready, and you will have it within an hour.

(The Commissioner.) I don't wish to hurry it on at
all. I only wished to know whether Mr. Stephen
had it with him or could give us the name of some
one who would give it.

(Professor Robertson.) There are some parties, I
believe, whom you propose to examine at 12 o'clock.
We can give this information either before or after
these witnesses have been examined.

(The Commissioner.) I think it would be conve-
nient to let these 12 o'clock witnesses be taken as they
come and get their examination over quickly, if that
is convenient for you.

(Professor Robertson.) That will be quite conve-
nient. I have here a statement of the weights, and if
you wish, Mr. Stephen could put it in now, instead of
calling another witness to put it in.

(Mr. Stephen.) I put in a table of the weights of
the ship and the position of the centre of gravity
(marked No. 11 of the inventory of productions).

(The Commissioner) (after examining the state-
ment.) I should like to say at this stage, Professor
Robertson, that while, of course, this information will
be subject necessarily to my examination, yet so far
as an opinion can be formed of it at the present
moment, it seems that the firm have shown the
utmost readiness to furnish me with the fullest in-
formation affecting the stability of the ship at the
time of the launch, which I regard as highly satis-
factory; and although there may be points upon
which I shall desire further information, even touch-
ing that question, so far as I can judge at the moment,
there is every disposition on the part of the firm to
put the Commission into the fullest possession of in-
formation for their guidance.

(Professor Robertson.) That is a very gratifying
statement, Sir Edward.

Mr. WILLIAM LAING, examined.

Professor Robertson.) Are you superintending
for the Glasgow and Londonderry Steam
Company?—I am.

What is the Company for which the "Daphne"
?—Yes.

Did you prepare the specification for the vessel?

What plans did you prepare?—The arrange-
ments of the vessel—the general arrangements.
Did you not prepare the design of the vessel?—
The model.

By whom was that prepared?—By Messrs.

Did you saw the vessel on the occasion of the
?—Yes.

Did you had seen her previously in the course
of?—Yes, I had.

Now, will you answer this question,—was she,
purely and simply the vessel as described in
specification?—She was built purely and simply
as described.

Were there any alterations upon your arrange-
ments?—Some very minor things, but of no
importance.

Did they did not interfere in any way with the
condition of the vessel?—No.

Now, I suppose I may take it from you that
nothing in the specification to suggest any
suspicion regarding the stability of the
?—No; on the contrary, I expected, and do
high amount of stability in the vessel. In
paragraph of the specification, one of the
was that she was to have a very high
of stability for carrying live stock without

65. You were present at the launch?—I was.

66. I think Mr. Brand, one of the partners of the
Company, was with you on the occasion?—Not beside
me. I don't know where he was.

67. He was near you, however?—I think he was.
He was on the same side, but it is only as to myself
that I can say.

68. Where were you standing when the launch took
place?—About midway on the length of the vessel,
on the starboard side.

69. Were you about half way down the slip?—
About half way down the vessel.

70. That was on the starboard side, the west side
of the vessel?—Yes.

71. (The Commissioner.) Were you on board the
vessel?—No, on shore.

72. (Professor Robertson.) You saw her going off
the ways?—Yes.

73. How did she take the water?—She seemed to
take the water just in the usual way. I did not see
anything particular about her.

74. There was nothing unusual in the way of her
taking the water?—Nothing unusual.

75. Did she go rapidly?—I think she went fully as
rapidly as any that I have seen, but I did not take
any special note of that or think very much about
it.

76. The movement did not at all strike you as
anything unusual?—It did not.

77. Did she take the water rightly?—Quite fair.

78. What did you see when she took the water?—
I saw her careen over to port, which I thought was
due to the strain put upon her by the port drag
anchor. She attempted to right after that.

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Mr.
W. Laing.

Mr.
W. Laing.
—
11 July 1883.

79. Did she attempt to right immediately after listing to the port side?—Yes.

80. You are quite satisfied that she made that effort to right herself?—Quite certain.

81. You were satisfied at the time that she was endeavouring to right herself?—I was perfectly satisfied of that.

82. What amount of time might elapse between her taking the water and going down?—That is a little difficult to answer. Perhaps a minute and a half or so, I really cannot say.

83. There was a good deal of confusion at the time?—Yes.

84. And excitement, I suppose?—Yes.

(*Professor Robertson.*) I have no more questions to ask this witness. Do you wish to ask him anything, Sir Edward?

85. (*The Commissioner.*) I should like to ask one or two questions. You say the vessel attempted to right herself?—Yes.

86. Do you think she returned to the upright position after listing to port?—I think not; but I cannot speak positively on that point. From the position I was standing in I could not observe that very well; but my impression is that she did not return quite to the upright.

87. You say that in your opinion at the time it appeared to you that the inclination to port, after she was afloat, was produced probably by the port anchor drag?—I thought so.

88. But was not the chain from that anchor led in through the hawse hole of the vessel?—No, it was not led through the hawse hole.

89. Was it not so at the port side?—I really cannot say, because I only noticed the starboard side. I cannot say very much about the port side.

90. On the starboard side the drag was connected to the side of the ship, and therefore had she gone over to the starboard side there might have been some explanation suggested possibly by that circumstance; but as the port drag went through the hawse hole, I am rather surprised to hear you suggest that that had anything to do with her going over?—I did not know that. I was not aware of that until this moment.

91. The company of which you are the superintending engineer owns other vessels?—Yes.

92. Does it own any vessels which, so far as you know, closely resemble this one?—The last vessel built has the same proportions of dimensions.

93. Has she the same general arrangements?—Very much so.

94. And the same type of machinery?—Yes.

95. And boilers?—Yes, the same type of boilers.

96. You said, as I understood, that you suggested the general arrangements of this vessel on behalf of your company to the builders?—Yes.

97. And they prepared the plans?—They made out the model and lines of the vessel.

98. Would you enlarge that evidence somewhat by explaining a little more fully how far you influenced the firm in preparing the design?—The only way in which I influenced them was by drawing plan outlines of the main deck, and a longitudinal vertical section showing the positions of the machinery, and the plans of the cabins and hold, and the masts, &c.

99. Who determined the power of the machinery and the type?—I did.

100. Did you make any large departures from your former practice in determining that?—No departure at all. The whole of the specification was made out, to a large extent, in accordance with former data and calculations, such as speed and power of boilers, &c.

101. Would you say that the vessel was behind time of delivery at all?—No.

102. Can you give the date when she was bound to be delivered?—I am not prepared to answer that. I don't know really what the date was.

103. Had you anything to do with the vessel except as an inspecting officer at the time of the launch?—Nothing whatever.

104. When was your last visit to the vessel by the launch?—Just immediately before the gang were cleared away.

105. It has been stated that the engines were complete, and completely attached to the vessel?—Is so.

106. Do you know if there was any considerable number of engineers and workmen of that class on board?—It did not strike me that there was any number. I really cannot say how many there probably be on board at the time. I did not notice that there was any number to strike me as being excessive.

107. Was not 10 days a very short time to allow for getting in boilers and completing the ship?—Because the machinery was all on board and complete. The shafting and everything was in. The vessel all in a general forward condition.

108. And you think that she might have been completed within the 10 days that were anticipated?—I think so.

109. You said that you expected the vessel to have rather more stability than usual, and you pointed out that it was mentioned in the specification that she being built for the carriage of cattle and live stock?—Yes.

110. Where would the cattle and live stock be carried?—On the main deck and in the 'tween deck.

111. Do you mean to say below the main deck?—Below the main deck, in the fore hold; that is usual practice—we carry them continually there in season.

112. There is no misunderstanding as to the 'tween deck, I presume. You speak of the deck above which the cattle ports open, as the main deck?—The 'tween deck is the deck immediately below the bridge deck. The main deck is the deck that forms the holds.

113. But the cattle ports are above what you call the main deck?—Yes.

114. I suppose cattle are carried also under the awning deck, on the main deck?—Yes.

115. Largely?—That is the principal place in which they are carried.

116. This vessel was intended for a well-known fixed service?—Yes.

117. Would she carry any heavy cargo below the main deck, or would she sometimes be expected to carry passages light?—They have often to make trips without any, or at least with very little cargo. Other companies do the same, of course.

118. And you believed that the vessel was adapted for the purpose, and not at all deficient in stability?—I did so, and I think so still. I am quite sure she will be able to do that.

119. Then, if you hold that opinion so strongly, you give me any idea of what, in your opinion, caused the accident?—I really cannot give any opinion upon that. I cannot satisfy myself as to the cause. The last vessel built, the "Briar," has exactly the same proportions of dimensions. This is the case of the steamers belonging to my company which have been built for the same character of trade. I produced a statement with regard to her (*marked No. 12 of the inventory of productions*). She is exactly of the same proportions of dimensions as the "Daphne."

120. Perhaps you had better add some information as to that vessel. I see that she was built in 1881. As to her performances since, has she performed the service satisfactorily?—Very satisfactorily indeed.

121. Did you happen to be at the launch of the "Briar"?—I was there.

122. Do you remember what state she was in regarding the machinery and boilers on board?—There was nothing but the shafting and propeller.

123. Does it strike you that there was nothing unusual in the condition of this vessel, the "Daphne," when launched, as to weights on board and people on board?—It did not strike me at the time.

124. And you, as an official of the owning company and an inspecting officer, and taking great interest

the launch, had no misgivings at all?—I had none whatever.

125. Did you influence in any way the arrangements of the launch?—Not in the slightest.

126. But were your duties confined to the inspection of the machinery?—They were confined to the inspection of the vessel as a whole, to see that we got what was necessary.

127. You were inspector for the whole order?—For the whole order.

128. Were you always there or merely a visiting inspector?—Just a visiting inspector.

129. Would it be any part of your duty to make yourself intimately acquainted with the details of the launch?—No.

130. Otherwise you would have known where the drags were?—I would have known, but as a rule that does not form part of our work at all.

131. You were on the starboard side of the ship?—Yes.

132. Did you give any information as to the various openings through the side of the ship as to whether they were closed or not?—None whatever.

133. You do not know anything about that?—I do not know anything about that except that I saw some of the ports closed, but that was simply casually. I do not know anything beyond that.

134. Perhaps you can give me some information as to the extent to which the deck was open for receiving the boilers, because it has been stated in evidence that plates beyond the limits of the boiler hatch were off the main deck in order to get the boiler down; was the deck very much open there?—On the starboard it was open, but I am not prepared to say how much. Messrs. Stephen will be best able to answer that. I cannot say to what extent, but it was on the starboard side they were open. The port side was intact. The main deck was all complete on the port side.

135. Can you not even say whether the starboard cattle ports were closed or open at the time of the launch?—I cannot say.

136. They are open now since the launch. It is to be presumed that they would have been open in any case since the launch; but I should have thought that, being on the starboard side of the vessel, and just half

way down the side of the vessel, you would have observed whether these ports were open or closed?—I did not notice that.

137. You saw the vessel when afloat and before she careened?—Yes.

138. Can you give us no information as to the extent to which the awning deck and the bridge of the vessel was occupied by people?—I noticed that there was a large number of people on the upper deck, but from the position I was standing in, I am not prepared to say how many there might be.

139. Do you remember where the gangway was by which people went on board up to the time of the launch?—Just at the wheelhouse.

140. On which side of the ship?—On the starboard side.

141. Did you observe many visitors at the vessel; I mean other than workpeople?—I do not know, I am sure.

142. You did not notice that?—No.

143. And you are not able to suggest anything at all in the construction or disposition of weights in this vessel, or the circumstances at the time of the launch, which would throw light upon this disaster?—I am not prepared to say anything upon that at all. I really could not satisfy myself as to the cause of it.

144. Suppose you were going to launch a similar vessel next week, would you suggest any difference in the mode of procedure?—I would take special precautions.

145. Such as what?—Perhaps closing the hatches or disposing of weights on the deck, and such things as that.

146. You would naturally do that after such an accident?—Yes.

147. You would perhaps also get rid of as much upper weight as you conveniently could?—Yes, certainly.

148. Did you ever launch any of your vessels with ballast in?—Never. We never required to do it.

149. Did you ever launch any before with engines or boilers on board?—Never.

150. But only with the propeller and shafting?—Just with the propeller.

Mr. ANDREW REID BRAND, examined.

151. (*Professor Robertson.*) You are one of the partners of the company for which this vessel was built?—I am a partner of the firm of Messrs. Alexander A. Laird and Co., managing owners of the Glasgow and Londonderry Steam Packet Company.

152. You were present in the yard on the occasion of the launch?—I was.

153. Where were you situated when the launch took place?—About one-third from the stern of the vessel on the starboard side.

154. (*The Commissioner.*) Were you on shore?—Yes.

155. (*Professor Robertson.*) You saw the vessel leave the ways and enter the water?—Yes.

156. Did you see her list to either side?—Immediately after taking the water she canted to the port side distinctly. Thereafter she made a strenuous effort and recovered herself to a very great degree. Thereafter she lurched over again to the port side.

157. (*The Commissioner.*) Do you think she came to the upright position when she attempted to recover herself?—I could not say for certain, but she made a very determined effort to right herself.

158. (*Professor Robertson.*) You are thoroughly satisfied of that?—Thoroughly satisfied. I was so satisfied of it that I thought the launch was to be a successful one, and that she had recovered herself so far.

159. (*The Commissioner.*) But she had wholly passed you?—She had wholly passed me, and I was looking at her in the water.

160. Was it difficult to see from your position whether she was upright or not?—I could not observe whether she was on the perpendicular or not.

(*Professor Robertson.*) I have no more questions to ask.

161. (*The Commissioner.*) You heard the questions that I put to Mr. Laing?—I did.

162. Can you add anything to your evidence on any of the points that I mentioned to him?—I don't think I can. My knowledge of the steamer was confined to one or two visits which I made to her. His statements with regard to the launch of the steamer are quite correct, and also as to the stability of our other boats, notably of the "Briar," which is an exceedingly stable vessel. We have been building a series of steamers during the last five or six years, and each one of them has proved herself thoroughly stable. They are employed in the general trade between England, Scotland, and Ireland, carrying passengers, cargo, and cattle, a trade wherein they require a high degree of stability.

163. Are they steady vessels at sea?—Exceedingly steady.

164. And have none of them given any indications within your knowledge, of tenderness, as we call it?—No, rather the reverse; quite the contrary.

165. Did you ever before see a vessel launched with so many people on board in proportion to her size?—I did not know the number of people on board. They were passing up and down the gangway continually—some going up and some coming out, and it was impossible for me to form any idea of the number on board.

166. Were you surprised after the launch to learn how many were on board?—I was surprised to learn that there were quite so many.

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167. Can you tell me whether the owners were quite satisfied with the progress of the vessel, or whether they were urging delivery?—We were so satisfied with the progress of the vessel, and the completion of the machinery, boilers, and so forth, that we quite expected that she could be delivered for us in a very short period after the launch. Everything was in a very forward state.

168. Had you any contract date for delivery?—No, there was no contract date. She was contracted for on 30th October last year, and was to be delivered in about 10 months, strikes excluded.

169. (*Professor Robertson.*) Then she was not overdue?—She was not overdue. She was not due until the end of August.

170. (*The Commissioner.*) That would be under a written agreement?—Yes, in a letter.

171. But you distinctly state that she was not overdue, and that you expected to receive her before the expiry of the 10 months?—Yes. There was no binding contract for the 10 months.

172. Was any inducement held out to the builders to quicken delivery: any premium offered them, or any advantage?—At one point we authorised Mr. Laing, if he saw fit, to mention to the Messrs. Stephen that if they worked later we would be quite prepared to assist them. We were quite aware that if the men work longer time there is a very serious increase of wages.

173. Then, although the builders had no reason, under the contract, for quickening the delivery, the owners were desirous of having her as soon as possible?—Certainly. We were desirous to have her with as rapid delivery as possible.

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Mr. ROBERT M'MASTER, examined.

181. (*Professor Robertson.*)—You are yard manager at Linthouse?—Yes.

182. You were present at the launch of the "Daphne"?—Yes.

183. Where were you standing at the time of the launch?—At the fore end of the ways on the starboard side, right underneath the christening gangway—right under the bows of the ship on the starboard side.

184. You were close to the fore end of the ways?—Yes.

185. And between them and the dogshores?—Between them and the dogshores.

186. Was this launch conducted in your usual way?—In every respect.

187. And with every care?—With every care.

188. You saw nothing to indicate or to suggest a doubt as to the accuracy of the launch?—Nothing.

189. How was the tide running at the time of the launch?—The tide was running up.

190. Did you see the vessel going into the water?—Yes.

191. Did she run right out?—No. When once she went half down the ways, her stern getting out well into the river, the flood tide commenced to slew her stern up the river. I was in a position where I could see that, and I noticed it specially. My attention was drawn to it.

192. (*The Commissioner.*) She was rather slewed across the ways?—Yes, with the flood tide.

193. (*Professor Robertson.*) When the drags passed you what did you do?—I stepped direct inside, close into the standing ways, so that I could get a particular view of the ship entering into the water.

194. Just describe her movement when she went into the water?—She went perfectly upright. She kept her perfect perpendicular and went right into the water without any motion whatever, and carried the drags right out until such time as the drags stopped her way on end, and then the weight of the chains forged her ahead until once the motion was entirely stopped of the vessel going ahead. Still, she

174. And you agreed in that case?—There was no agreement come to.

175. No, but you intimated through Mr. Laing that you would assist them if they worked longer hours?—No, we left it in Mr. Laing's hands to suggest to them that if it was thought prudent, we were quite willing to assist in getting delivery of the vessel, but Messrs. Stephen never accepted that offer. They never accepted any offer.

176. Do you know if Mr. Laing communicated that to the firm?—I understand he had a conversation with them, but perhaps Mr. Laing will explain that himself.

[*Mr. Laing.* What Mr. Brand has now stated is correct.

177. But the firm would hardly refuse?—They did not refuse, but the thing never went any further than that.

178. There was no definite understanding come to?—No, it was simply a talk.]

179. Then I presume in that state of the facts, if the vessel had been delivered much before her time, the owners would have been prepared to consider any request which the builders might make?—Yes, any increased outlay that Messrs. Stephen had incurred, because I saw men working late on a Saturday afternoon about a fortnight before.

180. So that, without any formal understanding, Messrs. Stephen might have reasonably expected that, in the event of their incurring a great increase of outlay for quick delivery, then a claim by them for some compensation for that, or an application to that effect, would have been fairly considered?—Certainly. We would have considered it perfectly reasonable on their part.

was slewing a little. Whenever the motion of her going ahead stopped, she seemed to list a little to port and gave a quiver as it were. That was the first indication I saw of her taking any list whatever.

195. Did you see her lurch to the port side?—Yes, a very little at the first.

196. Then what next?—A little more; that was the men, I suppose, sliding to leeward or going to the port side.

197. Did she carry the whole of the cradle with her or part of it?—The whole of it until once she was entirely clear of the ways, or well clear of the ways.

198. The whole of the cradle has been recovered?—Yes.

199. And is now in the yard?—Yes.

200. Did you see that brought ashore yourself?—I saw it in the river. I did not see it lifted entirely out of the river with the crane, but I saw it lying in the river and when it came ashore. I counted the pieces. We are in the habit of taking notes of the amount of wood that is put under a ship, and taking an exact note when it comes ashore again.

201. Who is your foreman caulker?—John Carrie.

202. Did you give him any instructions connected with the launching of the ship?—Yes. The instructions in that case were the same as in any other.

203. You gave him the usual instructions?—Yes.

203A. What were those instructions?—To close all ports, scuttles, and gangways, and to leave nothing that was not properly secured.

204. Who is the under foreman?—Neil M'Coll.

(*Professor Robertson.*) I may mention, Sir Edward, that he is one of the workmen whom you have expressed a wish to see before you to-day, and we have been exerting ourselves to bring him, and we hope to bring him up in compliance with your wishes.

205. What did M'Coll do?—He went round the ship and closed all ports and scuttles and gangways. I asked John Carrie when he came ashore if he had got everything carried out according to the instructions, and he said "Yes." I said, "Quite sure?" and he said "Yes." I had not seen Neil M'Coll, because

as still on board the ship, and he was going up the ship to the harbour.

6. Were the mooring pipes also closed?—Yes, a unusual thing. In this ship the mooring pipes to have slides, and, of course, we closed them as The water ports also were closed, and every- was closed.

7. Did you give any orders or instructions re- ing the drags?—I gave all the orders.

8. To whom did you give them?—To the fore- caulker.

9. Were your instructions and your orders d out?—Entirely.

10. In connection with this launch, did you do ing inconsistent with your previous practice in ing?—Nothing.

11. Everything was consistent with your previous ce?—Everything was in accordance with all our ous launches.

12. I now ask you this question—I have no doubt will be able to answer it. Was any trouble or spared in the yard to secure a successful h?—Nothing.

13. (*The Commissioner.*) When the tide took the up the river, I think you said that the stern of urch was past the end of the ways?—Yes, it ut into the river.

14. Well out into the river?—Yes, when the tide old of her stern she was well down in the river. ould be perhaps one-half, or fully one-half into iver, but still the fore end of her was on the on the end of the cradle.

15. Would not that have resulted in the stripping e ribbons from the bilge ways?—No, it depends w the ship is bearing. If she was a very heavy it would strip it, but if the weight is light, she herself. She has commenced to get water borne.

16. Have you had any examination made of the ways since they came on shore, to see whether was any stripping of the ribbons?—Yes.

17. Is any ascertained?—There could be nothing except that she had been rubbing hard. That ll.

18. Can you explain why the vessel was launched a considerable amount of flood tide left?—Well, would only be about a quarter-of-an-hour or nutes, I don't know even that. I could not give pecially the exact time when we launched her, se I am very busy for the last quarter of an hour the ship goes off. I have not got time to pick e exact time, and therefore I could not give you act time according to the time table, but, with nce to the river Clyde here, it is sometimes the hat the tide will hang perhaps half-an-hour or uarters of an hour before high water, and you naturally think it would flow no more; then it ome in with a rush and go up like a flood tide in perhaps three or four knots.

19. Do you think that state of things is special, or e emphasized at Linthouse than at other parts river?—I don't think so. I think they are all me.

20. They are all subject to the same thing?—All t to the same.

21. But that can only be due to causes in the cir- ances of the river which must be continually ing?—No, I attribute it more to the wind and er outside than anything else.

22. Then you merely mean to say that the time of ater is a little uncertain?—The time of high at Linthouse we always take at 10 minutes the time given at Broomielaw.

23. But you don't expect that the time table time e strictly conformed to?—No. Sometimes it is he time given in the time table, and sometimes ithin the time.

24. I don't know that you have mentioned how ou have been in the service of this firm as er?—As manager, I have been over seven

225. During that time you have launched a great many vessels?—About 97 ships of all sizes. That is as near an approximation as I can give. I have not given the matter particular attention, but that, I think, is about the number.

226. Do you generally launch a little before high water?—Yes. All shipbuilders on the river do so, because if anything takes place you have sometimes a minute or two to spare. Sometimes you have steamers passing up and down, and you have got to take a time when the river is actually clear, in order to save acci- dents. Perhaps you may be a few minutes earlier or a few minutes later. Sometimes you have large steamers coming up, and sometimes the vessel hangs on the ways.

227. With regard to the launching ways, sliding ways, bilge ways, and cradle of this vessel, were they such as you have used for much larger vessels?— Yes.

228. So that you consider them as beyond all ques- tion sufficient for the efficient launching of this small vessel?—Yes; they would launch a vessel three times the size.

229. They seemed to me as being sufficient to launch a vessel very much larger than she was. With regard to the declivity that you gave to the sliding ways, did you determine that?—Yes.

230. Was that in accordance with the usual practice? —It was in accordance with our usual practice, and a little more than for a light ship on the stern. It would be $\frac{1}{4}$ ths on a heavy ship, and I would give her about $\frac{1}{8}$ ths.

231. Did you consider that she went off at any exceptional speed?—No.

232. There was nothing unusual as regards that?— No.

233. Do I understand you to say that you consider that the drags entirely took out of the vessel the velocity acquired by the launch, and stopped her and brought her back ahead?—They entirely stopped her after she had gone her given distance. She took the drags with her.

234. I suppose you expected her to carry these drags?—Yes. We always expect her to do that, because if we overload them she would burst them and take everything away with her.

235. And do you consider that she was upright until the motion was taken out of her by the drags? —Entirely.

236. And that no careening began until after the motion sternwards had stopped until the ship was entirely settled in the water?—With the exception of the drifting with the tide up.

237. Do you think you could be confident of that from your own observation?—Yes, from the position I was standing in. I am giving it to you as a matter of conscience.

238. With regard to the closing of the side scuttles, and ports, and so on, I understand you to say that you gave specific orders to Mr. Carrie, chief foreman of caulkers, to close all ports and have them perfectly secure, and that Neil M'Coll, the next foreman of caulkers, was within your knowledge instructed to carry out your orders?—Yes, and also to tell the foremen in their respective places, to see that their men were not to open any port or window, until the ship was moored at the Broomielaw.

239. But you are unable from your own knowledge to say whether these orders were carried out?—I can only speak to a certain extent. I cannot speak to it on the whole. I can only speak to what I saw myself. I did not go round the whole vessel, because I had entire confidence in John Carrie, and I intercepted him coming down off the gangway and asked him if these directions were carried out faithfully, and he said everything was carried out. I went to the star- board side myself to see that everything was done.

240. Would that apply to the water ports at all?— Everything where there was an opening.

241. Returning for the moment to the drags, do you confirm the evidence that the port drag chain was

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taken through the hawse pipe hole of the ship and carried over the windlass down to the clench at the keel?—Yes, that was the way it was done.

242. And is it your opinion that a drag so circumstanced could contribute to the capsizing of the vessel?—No, it could not do that.

243. You don't think it could?—No, it could not do it. It has no leverage on the ship whatever; it is right at the extreme end.

244. But had it happened that the port drag had been exchanged for the starboard drag, and the starboard drag for the port, which, I suppose, could not have happened in the circumstances, but had it so happened, then you would have held a different view?—Yes, certainly. That might have happened, because it was at the extreme limit of the breadth of the ship, and had more lever power.

245. But it is clearly your opinion that the drag chains did not contribute to the capsizing, but that, if they had any influence on the question, it must have been in checking them by virtue of the action of the drag on the starboard side?—That is not my opinion.

246. You don't think that?—No. My opinion is that the ship was entirely tripped by the flood tide coming up and the current coming down. That is my opinion of the whole matter—that the ship gave a slight quiver to leeward, and the men were sent off their equilibrium and rushed down to the lee side as it were.

247. In other words, you consider that the first inclination was given by tidal causes, and that the ship was unable to recover herself from what occurred afterwards on board?—Yes.

248. Now, can you give me any opinion as to the weights on board. In the first place, have you ever seen a vessel of this description launched before under the same conditions, with engines in and boilers not in?—Well, really, I have not.

249. You don't remember seeing that case?—Not exactly that case, but our invariable way of launching is with engines and boilers both in together.

250. I don't wish to press you on any point, so as to lead you into evidence that you are not confident about; but have you formed any opinion as to what would have been the effect as regards stability of having the boilers on board?—No.

251. You have not considered that point at all?—No.

252. Have you been in the habit of having so large a number of men—of workmen engaged at their work, on board during a launch?—We always had as many men as would finish the work in the given time. Our given time after we launch is 10 days, and we had just about the same number of men in proportion on board this ship according to the work that was to be done as we usually have.

253. Where do your ships generally go after launching?—To the Clyde Trustees' crane.

254. Do you usually put on board during the launch, or before the launch, all the materials that are prepared or that are assigned to the vessel in the building yard before she is launched, so as to have them on board?—As near as possible. There is always wood of some kind or other, and fittings here and there that cannot be got ready in time, and it was the same in this case. A number of things for this ship were not on board also. Of course they are put on board as far as we can for handiness to the workmen.

254A. And do you usually—you must not suppose that I am implying anything in the form of my question, I am only seeking special information in this case—but do you usually launch with all the workmen engaged on the ship still working on board at the time?—Yes; we are never in the habit of putting any one ashore.

255. With the ship so far advanced as this vessel was, with the engines actually in place and fastened, and the boilers elsewhere ready to go in, what iron plates should there have been on board this vessel either loose or lashed?—The plates on board were some of the boiler combings on the bridge deck and

the galley, the deck plates and those that went the tops of the boilers for riveting down the combings. These were on the bridge deck. The main deck of the ship was also the same. As it was removed was placed on its respective so that there was nothing added, as it were nothing taken away.

256. Now, this is a point of very great importance. Are you perfectly positive that the plates—were not in place, because the boilers had to go on board, and we will take the combings round hatch—were the materials for these purposes on the where they would have been riveted up had they been completed?—Yes, as near as I could see. They might be shifted forward or aft as it were.

257. But they were on the same decks?—(Same decks. The men might shift them a little I left? I was not standing by them steady.)

258. Did you give any instructions as to where they should be?—I gave instructions when the men commenced to take them down to keep everything on its respective deck, as near hand the place as possible, and re-covering up again.

259. Can you be quite clear in stating that there were no plates—confining the question to plate board there, other than those intended to be riveted into place, or fitted into place after the boilers were in?—None whatever; I am quite confident on that point.

260. There were no plates on board for any other purpose?—No other purpose.

261. You are quite certain of that?—Quite certain. All the iron work was entirely complete, and had only to be removed; they were not riveted.

262. Can you say anything about any timber on board? What timber was there loose on board? The timber that might be loose was some deck planks over these respective places, where the beam stringers had to be removed to allow the boilers to be taken in.

(Professor Robertson.) At this stage I may say to you some unnecessary trouble if I put in a statement which has been handed to me, a paper titled "Estimate of Materials on Board the 'Daphne' at the time of Launch." There is not only a statement here of the materials, but also a statement of the particular places where these materials were lying at the time of launch.

(The Commissioner.) I may say that I shall be glad to have that statement, but I think it would be better to supersede the questions I am now putting.

(Professor Robertson.) Pardon me, I think perhaps it might rather have aided you. (Paper in, and marked No. 13 of inventory of production.)

263. (The Commissioner.) The information will no doubt be very valuable, but I should like to bring out in evidence what I am now asking. Do you know of no timber or timber materials other than the deck planks which were necessarily removed to make place to receive the boilers?—There might be joiners' lining boards, but these were very light and very little of them.

264. What about the cattle fittings?—The fittings are all fitted up and of course remain in their place.

265. But were not any of them taken down?—They were taken down only in that place where they were to come in.

265A. All the rest of the cattle fittings were in place?—Yes. There might be a few on the port removed from there and put in the horse stalls for anything I know. I could not be positive on that point, because, when men are working in a place they shift things to suit themselves, and of course, you cannot always be on the very job.

266. Who would be able to speak to that?—I cannot say, because men will shift things at all times.

267. Can you give me any information about the davits of the vessel?—They were shipped on the side and they were lashed on the starboard side at their respective places.

268. We have not said anything about those materials, whether they were loose or lashed. Would you have considered it necessary to lash them at the launching?—Anything that was likely to shift was lashed, such as the davits, they were lashed, and the davit sockets and timber heads.

269. Do you from your experience in launching expect a certain amount of rolling motion to occur in a vessel after she leaves the slip?—When once she gets into the water there is always less or more. In some there is more, while others are quieter. You cannot always explain why it takes that motion.

270. You consider that there is always liability to some little rolling after the vessel is in the water?—Yes.

271. And you provide for this by making everything secure?—Yes, as far as possible.

272. Now, can you give me any information about the number of men employed on the ship at the time, and the number on board?—I could not from my own personal observation. I could not say further than what I have heard others state on the subject. I could not form any opinion of my own.

273. The books of the firm would show the men employed upon the ship?—Yes.

274. But they would hardly show who were not on board at the time. You have no means yourself of giving information on that point?—I could not do so with any certainty. It would only be an approximation. It might be within the range or it might be overstated.

275. Can you tell me this, whether the launch of the vessel caused you, as the manager, to give any orders for men to remain on shore, or whether you allowed the work of the vessel to go on without interference?—The only thing that I myself did was, there were a number of men and boys who were not started to their work that day, and I went up to the head of the gangway and drove every man down from the head to the bottom. They said they were going on board the ship to work, and I said, "I do not care where you work, you can travel to the quay," because there were a number of hands all going on board purposely to get the passage up on board the ship, and I drove all and sundry down once or twice. I had no hesitation in doing so, because they were obstructing the other men there at work by having such a number of hands on board.

275A. The people on the main deck of this ship during the launching would not see anything of the launch, because the sides of the ship came up above their heads?—Yes.

276. Therefore the natural expectation would be that the people on board at the time of the launch would go where they could see something, and get up on the awning deck?—That is what they always do.

277. Now, what was the appearance of the vessel as regards the number of people on the awning deck and on the bridge deck when you saw her afloat. Did she seem crowded there to you?—The hands all seemed to be mostly to the port side.

278. Were you in a position from which you could see any movement of the people on board after the careening began?—I could see them only on the fore-castle head, and I saw them sliding to port then. I could not see anywhere else, because she was drifted by that time, and slewed in the water out of my line.

279. You were the manager of the works, and of course responsible in a very high degree to the firm, no doubt, for conducting the launch safely and satisfactorily. Did anything occur to you before the launch as being likely to occur under the circumstances which would try the ship as regards her stability with so many men on board and so light, and so forth?—No.

280. Nothing occurred to you?—No; not the slightest doubt ever occurred to my mind.

281. If it had, I suppose you would have taken steps accordingly?—Certainly.

282. As I gather from you, Mr. M'Master, you have formed an opinion as to the cause of the accident,

and your opinion is, that launching as you did, and as you usually do, with a greater or less rising tide, there was something in the tidal conditions which gave the ship an unusual list after she was afloat, and that owing to the number of persons, and the quantity of things on board the ship, she was unable to recover herself, and went over. That is your view of the accident?—Yes.

283. Of course, it must have been a very appalling thing to you to see the accident?—Certainly.

284. And I should like to ask you, if you feel disposed to answer the question, what you would do suppose you were going to repeat the launch of the vessel; I do not mean under the natural repugnance to run the slightest shadow of a risk which you would feel after such an event, but what would be your general inference as to the precautions to be taken in launching a vessel of this description? In the first place, would you make any difference in your practice with regard to launching at a given time of tide?—Well, I would rather not answer that question until I get a little time to consider it. It is just rather hurriedly put to me, but I had not the slightest hesitation, and I have none yet, but what the "Daphne" was quite safe, and stable enough, if the ship had got the chance of recovering herself.

285. Then the fulness with which the firm has supplied information, and the readiness which every one has shown to answer questions, prevents me, even if I had the disposition, from pressing any opinions from gentlemen on any matters which are matters of opinion, but it just seems to me, from your point of view, that the old fashioned desire of launching as near the top of high water as possible has been a very wise practice?—Certainly; it is the safest.

286. I think you said you were not quite certain as to the actual time of the launch. I could not say to a few minutes. I think it would be from 20 to 25 minutes past 11, but I could not definitely say. It would be about 11 o'clock or 5 minutes past 11 when I last looked my watch. The hands were ordered to take away the bilge blocks.

287. Were you the person who gave the order to knock away the dog shores at the launching of the ship?—I passed the word for Mr. John Stephen. Mr. John stands right in the centre line of the ship, so that he can see both sets of dog shores. He stands exactly at the stem of the ship, and he can see me on the starboard side and the head foreman carpenter on the port side. When we have all ready and the men standing by, we pass the word "make ready to take away," and when Mr. Stephen says "down logs," we knock away.

288. Whoever does this, whether you or Mr. John Stephen, he has to attend, of course, to several matters which would interfere with the launching of the vessel at the precise moment. For example, the river must be clear?—We have hands stationed there to pass the word along.

289. Have you any means of keeping it clear?—Not further than putting out a boat, both above the launch and below the launch, with the word "launch" on a red flag; and it is the rule that no ship is to pass up or down, but some will persist in passing even when the notice flag is up.

290. Can you say whether there is any power for the builder who is launching a ship to put in force any kind of authority to keep the river clear for any time?—No, not that I am aware of.

291. Then he has to watch his opportunity?—Exactly.

292. And to take all the precautions he can to prevent accidents?—Yes.

293. Therefore, on the Clyde, as on other rivers, what may be called the accidental conditions of the traffic at the time have some influence upon the exact time of launching?—Yes.

294. So that you could not launch at a precise moment if you chose. You might have a vessel in the way, and you might be obliged to wait until she passed?—Yes.

Mr.
R. M'Master.
11 July 1883.

J. Carrie.

1 July 1888.

398. That is your evidence?—Yes.

(Professor Robertson.) Sir Edward, in the letter which you instructed Mr. Lamond to write to me yesterday afternoon, mention is made of Neil McColl as one of the men whom you wished to examine

personally. I have never seen him, and consequently I have not precognosced him, but I understand he is now in attendance, and accordingly I shall present him in order that you may put such questions to him as you may deem proper.

N. McColl.

NEIL MCCOLL, examined.

399. (The Commissioner.) You were serving at the time of the launch under Mr. Carrie, were you?—Yes.

400. What part did you take in the closing of the scuttles and ports and cattle doors?—I had always charge of closing the ports. Some call them ports and some windows, but I mean the windows in all ships, and I did close all the windows in this steamer. I saw them all closed.

401. Did you take any one with you to do the work of closing them?—Yes. Shortly after breakfast time I took an apprentice with me and I told him to go down to the cabin and close all the windows there. I went myself to the steerage down below, and I closed them all there with my own hands. Then I came up into the rooms above, and I went up and closed them all there. Then I went back to the cabin to see if the apprentice had closed them there. I went down and saw that they were all closed, and I told two foremen joiners there, Crawford Dick and Andrew Eadie,—I told Crawford Dick that he was not to let any of his men open the windows until after the launch, and immediately John Carrie came down, and I came up and left Carrie down in the cabin.

402. Did you see to the large cattle ports or doors?—Yes. There were four of them. There are two on the starboard side. They were all secured, I believe, before breakfast, and the one in the port side forward was secured by Cowie, a foreman carpenter. I was down and I saw that the one aft on the port side was not secured, and I went away and got two pieces of rope. There were two rings in the door, which was in four parts. There was a door fore and aft across, and we slipped it down between the two halves and kept these together. The fastening of the door in that way would keep the whole door closed. I passed the rope through and tied it to a stanchion that was below the upper deck, one of these cattle stanchions, and secured the port.

403. How long before the launch was it when you last saw these cattle ports, do you think?—Pretty shortly before. Perhaps about half-an-hour, or say three-quarters of an hour.

404. Have you any reason to believe that they were opened afterwards?—No.

405. Did you ever hear anyone say they were opened?—No, they were not opened. They could not have been.

406. You feel sure they were not opened?—They could not be opened, because it was such a short time.

407. You have described how the forward one was fastened. How was the foremost one in the port side fastened?—The slip bolts I believe were in the two lower halves, and Mr. Cowie, the foreman carpenter, fastened them with wood; I could not exactly state the very way the fore port was fastened.

408. Do you think it at all probable that any workmen would come afterwards and unfasten them?—I don't think it; they had no reason to do it.

409. What about the water ports; did you see to the fastening of them or did you leave them to swing?—They were very small ports and they were in the main deck—the same deck as the hatches of the boiler hold. I did not take much notice of these, I must say. They were very small.

410. Where were you at the time of the launch?—I was standing with my back to the aft end of the engine skylight up on the spar deck.

411. In going all about the vessel you must have seen the plates that were for the deck, and iron coamings for the hatchways, and so forth, lying about, did

you?—Yes. I was aware the boiler was to go down. The plates and a good deal of iron was put on the starboard side. I did not see any on the port side. All that I saw was on the starboard side.

412. Was that lashed in any way or laid loosely?—I could not say.

413. You don't know?—I don't mind.

414. You did not observe that?—I did not.

415. Did you observe any timber materials lying about?—I noticed a few deck planks on the port side. I think that was the most of what I saw.

416. Was it in pursuance of your orders that you were launched on board the ship? Did your duty keep you on board the ship at the time of the launch, or did you remain on board by yourself?—No; I was only on board that boat a very short time. I was at the quay at another boat, and I went and took my dinner and went down to her. That was all the time I was in her until she was launched.

417. You don't understand my question. Were you launched in her as a matter of duty, in pursuance of your work, or were you launched in her by choice? Could you have come on shore before the launch if you had chosen?—Yes, if I had chosen; but I had to go away with the boat—that was my duty.

418. Then you had to be on board of her if you had to go away in her?—Yes.

419. You have been at many launches before?—Yes, I believe I have been at 100.

420. Have you often been launched in vessels before?—Yes, I believe I was in above 100 launches with the same firm.

421. Was there anything in the condition of this vessel and these plates, and timber material, and the position of the engines, that caused you any sort of alarm, or to take any notice of these being dangerous in any way?—No.

422. You had no apprehension of any accident at the time?—No, the only thing that I was afraid for was, that there were too many people on board, a large top weight of men, such a crowd of people.

423. Where were the people at the time of the launch?—They were in all parts on the top.

424. Were there any on the main deck?—I could not say for the main deck. I was on the top on the spar deck, and they were all along the spar deck on both sides, and a great many on the poop,—thick.

425. That was the only thing that struck you as being exceptional?—Yes.

426. When the ship capsized, or when she was capsizing, did you notice any of these plates or timber material shift?—No.

427. You saw nothing shift when she was capsizing?—No. I did not notice anything. There was nothing shifted where I was. I could not see what was below. I was on the spar deck on the top, so that I could not see what was below on the low deck.

428. But there were some plates and materials on the spar deck, were there not?—I did not notice any where I was standing.

429. Did you notice any cables on board?—No.

430. But there were cables on board?—I did not take notice. I was only a short time in the boat.

431. You saw the men shifting, of course, when the vessel capsized?—Yes.

432. Did you notice the vessel list to port first, and then come back?—She might have come back, but I cannot recollect very well.

433. You could not say whether she simply rolled over at once, or whether she made an oscillation back and then over?—I cannot say.

434. What happened to yourself when she leaned over?—I was thrown into the water.

435. Did you hurt yourself anywhere?—Yes. I got one of my legs skinned and a small blow on my left arm, and my right arm was strained, but there was no mark on it.

ALEXANDER KING COWIE, examined.

438. (*The Commissioner.*) Would you tell us your employment?—Foreman shipwright.

439. Were you on board the "Daphne" at the time of the launch?—Yes.

440. Where were you?—On the starboard side before the gangway. I was standing by the starboard end of the fore winch.

441. How long were you on board before the launch?—I was on board all the morning.

441A. What were your duties there?—Looking after the shipwrights, and getting everything secured before the launch.

441B. You say getting everything secured before the launch; what sort of things secured?—Getting some material on board that we would require when we got up to the quay—any light things.

442. From whom did you receive your instructions?—From the head shipwright.

443. Who is he?—Colin Wallace.

444. Had you any special instructions that day as to getting things on board?—No, nothing more than ordinary.

445. In the ordinary manner you got on board all that you knew belonged to the vessel?—Yes.

446. Can you say whether any of the materials that were not in their places—that were loose, in a sense—were secured before the launch so that they could not shift if the vessel careened?—The cattle doors on the starboard side were all secured.

447. Were there any iron plates?—No, not in the way there.

448. But there were parts of the deck plating, and some of the iron hatchway combings, it has been stated, that were not in their places, and that were on board?—I did not take any notice of them. I only looked after the wood part.

448A. How were these cattle doors secured?—On the starboard side we had horse stalls amidships, and there were doors and hinges on them, and in case of any of them getting away out of the way we put them in there in midships.

449. That was done rather to prevent them being lost than in anticipation of any accident?—Yes; to prevent losing the run of them.

450. Had you any idea yourself of the desirability of securing loose materials with reference to the launch itself?—No; nothing particular.

451. It would not occur to you that at the launch these things would be likely to fetch away and to cause trouble?—No.

452. You would have acquaintance with all the timber materials on board; would you?—On the main deck.

453. Had you any charge of cabin fittings or anything of that kind?—No.

454. Could you form any rough estimate of the timber materials on board and not in place?—I could not exactly tell you the weight of them. We had not a great deal. There were simply a few pitch pine planks, and they were quite secure. They could not go to the port side at all. The stanchions of the gangways would keep them from going over to port.

455. As to the people on board, how many people were under your orders?—I had 12 altogether.

456. Have you any idea how many people there were on board altogether?—No, I could not give you the least estimation of that.

436. Was that on your way to the water, or after you were in the water?—It must have been when I was thrown into the water.

437. Do you feel confident that the cattle doors and the side scuttles were all closed and secured?—Yes, they were all closed.

(*Professor Robertson.*) Mr. Lamond's letter made mention also of Alexander King Cowie, as one of the persons whom you wished to examine. I believe he is also in attendance, and, if so, he will be called.

N. McColl.

11 July 1888

A. K. Cowie

457. You stood on the spar deck, you say?—Yes.

458. It has been stated in evidence that there were a large number of people on the spar deck at the time of the launch?—Yes.

459. You must have seen them?—I saw a number of people on the deck.

460. Did it strike you as being a large number?—Yes, there were a great number of people on it; but nothing more than I thought was belonging to the vessel. I did not see any strangers or anything of that kind. It is a duty of ours to put everybody not connected with the ship ashore at a launch.

461. That was the understanding?—We always do that. We had no understanding with regard to that day.

462. But I understood from Mr. McMaster there was no general rule to that effect, and no restriction placed upon people getting on board?—The only thing was that they left them on board, but we do not like them to come on board, because they will be in the way of anything; so we do not wish to have them on board at all.

463. I suppose there were no men actually at work at the time of the launch?—Not that I am aware of.

464. Can you give no evidence as to whether there were any of the men who were on board on the main deck at the time of the launch?—No.

465. Had you anything to do with making fast any of the ports or cattle doors or side lights?—No, it is the caulker's duty, but I assisted him in doing it.

466. Would you speak a little more fully on that point, as to what you did?—On the starboard side I took notice that at the two lower ports there were two slip bolts, and at the upper port there is a hinge, which was closed up and a sort of fastened to them. It came through like a staple, and there was a half-inch bolt put in to secure it, so that there would be no fear of it opening out. I took part in doing it.

467. Was that on one side of the ship only, or on both?—On the starboard side.

468. But did you not help to make fast the door on the port side as well?—On the port side the two slip bolts were on the lower part of the door. On the upper part the slip bolts were not on, so that we got two pieces of plank for each of the halves, and put them up there and toggled them, so that it would not give way.

469. You did that on the port side?—On the port forward, but I did not see the aft one.

470. Then you know that the foremost one was made fast?—Yes.

471. Had you any reason to suppose that that was interfered with afterwards?—I did not see it interfered with, and I was standing by it until the launch.

472. Do you know anything about the closing of the side scuttles?—I saw they were shut, but that is all.

473. Can you say anything with regard to the actual extent to which the ship rolled first to port and then came back to starboard, and then rolled over to port, or what was your impression?—The vessel cleared the ways thoroughly, and listed to port and righted herself a bit, and I thought she was all right then.

474. Was there anything alarming in her first lurch to port?—No, I did not see anything.

A. K. Cowie.
11 July 1883.

475. But was it more than you expected?—No; I did not expect anything of the kind.
475A. It is not an unusual thing for a vessel to roll over a little?—Sometimes they take a small lurch.
476. Did you see anything shift on the inclining of the vessel?—No.
477. You saw no material move?—No, none whatever.
478. But you saw plenty of people move?—Yes; after she took the second lurch I saw the people all in the water then.
478A. Did you see any movement of people after the first lurch?—Not on the starboard side. The people were right round both sides.
479. Did you see any movement at all? Was not the first lurch sufficient to cause people to run or tend to run down to port?—No, I did not take any notice of that. It was the second time that I noticed it.
480. Are you perfectly clear on that point, that after the first lurch there was no movement of people?

W. Gouk.

WILLIAM GOUK, examined.

487. (*The Commissioner.*) You are a foreman ship carpenter?—Yes.
487A. How long have you been with Messrs. Stephen and Sons?—Eleven years.
488. You had charge of the launching arrangements?—Yes, making up the ways at the fore-end.
489. You were not launched in the ship?—No.
490. What did you observe in the movements of the ship after the launch? Where were you at the time of the launch?—I was standing about 30 feet from the fore-end of the ways.
491. What did you see of the ship as to her rolling?—When she left the ways she listed to port, and then righted a little again, and then went over to port again.
492. You have expressed an opinion that there was rather a large number of persons on board, have you not?—No, I have not.
493. Are you sure you have not?—Yes.
494. Have you any idea how many persons there were there?—No.
495. You have no notion?—No.
496. Did you see people going up and down from where you were?—Yes. I saw people going up and down the gangway into the vessel, but the number I don't know.
497. What did you do when you saw the vessel likely to capsize?—I called to a number of people, and went to get a boat to get into in order to see if I could do anything to save anybody.
498. That was before the vessel actually went over? She was over on her side by that time.
499. It was not at the time of her first lurch that you did that?—No.
500. It was not until you saw that she was certain to go?—It was not until the second lurch.
501. When you saw that she was really capsizing?—Yes.
502. What sort of rate did she go over at; did she go over fast or slowly?—Slowly. At the last lurch she went over very slowly.
503. It was not when the first lurch came that you went for the boat?—No.
503A. You are clear upon that?—Yes.
504. Were you ever on board the "Daphne"?—Never.
505. You did not actually see her finally capsize, did you?—No.
506. You had gone for the boat?—Yes.
507. I can only ask you again whether you are certain that you have not expressed an opinion as to the ship being unduly crowded with persons?—I don't know the number of people that were on board of her.

- I did not take any notice of any movement of the people then.
481. Have you any opinion as to why she capsized?—None whatever.
482. Do you know anything about the drag chains?—No; I did not see them.
483. And you have no opinion at all about them?—No.
484. You would not know what to do next time if you had power to order things?—No, I could not say.
485. Do you mean to say that, supposing you were going to launch a ship on your own responsibility, you would have everything exactly the same on board as it was in this case?—Yes.
486. Only you would stop on shore yourself, I suppose?—No; if it was my duty to be on board I would be there.
(*Professor Robertson.*) The next witness is William Gouk, foreman ship carpenter, whose name is one of those mentioned by Mr. Lamond in his letter.

508. But do you say that you never have expressed that opinion?—Yes; I never have expressed that opinion.
509. Well, I shall not press you any further; that is all I wish to ask.
(*Professor Robertson.*) Among the names mentioned by you, Sir Edward, is Robert Scott, carpenter. I believe he is also in attendance.
(*The Commissioner.*) I am sorry, Dr. Robertson, to call any witness we don't really require, and looking to the nature of the evidence we are getting, and the very limited character of Scott's evidence, I don't propose to call him. I will not trouble you with him.
(*Professor Robertson.*) Then, is there any other name that you wish called?
(*The Commissioner.*) Yes. I should like you to let me know who are here.
(*Professor Robertson.*) The names furnished in Mr. Lamond's letter are 12 in number. There is first Mr. McMaster, who has been examined. There is one Henry Farrell, who is present, I understand. Robert Scott you have expressed a wish not to have called.
(*The Commissioner.*) We will dispense with him, but I should like to ask a question or two of Farrell.
(*Professor Robertson.*) He is asked to attend and has promised to come, but the gentlemen present do not know him by sight. However, we shall make every effort to bring him.
(*The Commissioner.*) I am much obliged. I may just say at this point that as there was no offer of evidence yesterday other than that which the building firm tendered, I thought it right to avail myself of such information as I had as to persons who had expressed opinions touching points of importance in the inquiry, with the desire that nothing might be wanting on my part to give any one who had any strong opinion or any information to contribute the opportunity of offering it. At present I am afraid we are not getting any considerable addition to our information from these witnesses, and, therefore, I propose to ask the others that come only questions upon those points which, from the facts before me, I am led to believe they have expressed opinions upon. I would say that the evidence we have already had has cleared up several of the points and made it unnecessary for me to call any further evidence touching the launching ways, for instance, and the launching arrangements. That seems to have been entirely got past. On the other points a good deal of light has been already thrown, and I should like just to have the opportunity of giving these various witnesses the offer of making any statements which they may think fit, based, as I say, upon information which has come to my knowledge, and, therefore, I will question them briefly as they offer themselves.

HENRY FARRELL, examined.

H. Farrell.

11 July 1883.

510. (*The Commissioner.*) What are you?—I am a rigger's labourer.

511. Where were you at the time of the launch?—At the stern.

512. On board the ship?—Yes.

513. How long were you on board the ship before the launch?—I was up and down in her pretty often. I had some work doing on her.

514. But when the last time before the launch were you on her? About an hour or half-an-hour?—About a quarter-of-an-hour.

515. Did you observe any iron plates or timber materials lying about the deck of the vessel?—I paid no particular attention to anything.

516. I did not ask you whether you paid any particular attention, but whether you saw anything?—I was at my work, and I did not see anything or look for anything.

517. I did not ask you if you looked for anything. We often see things when not looking for them. Did

you see any loose material lying about the decks of the vessel?—No.

518. Have you stated that you did see loose material?—No.

519. Have you not stated that you did see loose wood and materials lying about the decks?—No.

520. Have you expressed any opinion as to the number of people on board?—Yes. Some people asked me before about that, and I said I could not tell, but by what I heard and saw some people said there were about 200 on board.

521. Have you not expressed any opinion as to there being a very large number of people on board for the size of the vessel?—No.

522. Have you anything that you wish to say? You were one of those men who were thrown into the water, I suppose?—Yes.

523. Your life was risked at the time?—Yes.

524. Have you anything to say at all tending to throw any light upon the accident?—No.

JAMES M'LEAN, examined.

J. M'Lean.

525. (*The Commissioner.*) Are you a joiner?—Yes.

526. Where were you working at the time of the launch?—On the top of the skylight of the engine room.

527. Did you see any loose plates lying about or stowed anywhere on the deck?—There were two or three loose plates, but they were fixed to the combings.

528. Then they could not be loose if they were fixed?—They were lying against the combings—they had just been lifted up at the time.

529. On what side?—The starboard side.

530. Therefore the combings would prevent them going to port?—They could not shift.

531. Where did these plates belong to?—They were just lifted up beside the boiler space off the deck.

532. You have expressed some opinions as to the vessel having a great speed on her at the time of the launch, have you not? You thought she was very fast in her movement?—I thought that, but it is some 15 years since I was at a launch before, so I told them at the time I expressed that opinion that I had no experience.

533. You would not be disposed to maintain it in the face of a good deal of evidence that the launch was quite usual in that respect?—I could not tell.

534. You do not wish to call that in question?—No.

535. Would you describe to me what you observed of the motions of the vessel after she got afloat?—I felt that she went steady until she reached the water. I felt a quiver then, and she went to the port side. I thought several times that she would have come back, and she came, I think, four or five inches, as far as I could recognise at the time.

536. Then you formed the opinion, that when she first began to careen, she went right over until the gunwale was nearly under? She did not make a slight roll in the first instance and then come back again?—I don't think she came back.

537. Your impression is that she did not come back?—She might a little, but very little, I thought.

538. So that it was rather one general careening motion, but with a little check at one point?—Yes, you felt the quiver as she was going over.

539. What did you do when she was capsizing?—I made for the iron supports for the small boats and got on her broadside. I climbed over the top of the skylight.

540. You got on from the top of the spar deck to the side of the ship?—Yes.

541. What happened you then?—I saw a tug at the after end and made for her, but could not reach her in time, so I was thrown into the water then.

542. What do you think of the number of men on board the vessel at the time?—I only went out in the afternoon before, and I am never on board of a vessel, so that I can scarcely say. I think there would be about 200 at the time or more, betwixt that and 250.

543. You said you thought them rather a large number for such a small vessel, did you not?—There were so many running ashore and coming aboard before she was launched that I could not say how many there were, but I thought it would be something about that.

544. Of course, I don't expect that you, in the circumstances of the accident, attempted to form anything like an estimate of the number of men who were on board when you yourself were saving your own life in danger, but have you not expressed an opinion that the impression made upon your mind before the capsizing was that there was a very crowded state of that spar deck; have you not said that?—No; I don't think the spar deck was overcrowded at the time. There were more men on board, I believe, than usual, I thought that.

545. But you have not been launched in a vessel for 15 years?—No.

546. So that you could not even on that point speak from recent experience?—No. I could not give a decided answer on that.

547. It is rather strange that one after the other of the witnesses who were upon the spar deck of a vessel with something like admittedly 200 people on it at the time should all somehow or other come and say that they did not notice any particular number of men, and have no idea of how many men were there, and whether the deck was crowded or anything like it. Now, surely you being there, and being an intelligent man, must have had some impression formed in your mind whether the deck was crowded or not, where it was most crowded, and other facts of that kind?—Well, it is a thing when a man is at work that he does not notice; at least, I know for my own part that I never think of what is going on round about me.

548. But you were not at work?—I was at work.

549. Not at the time of the launch?—I was at work until the shores had been knocked away, and then I looked round.

550. Then you did take a look round?—Yes, but it was not on the deck I looked.

551. Do you mean to say that you did not observe whether there was a considerable crowd of persons round about you, and near where you were?—I could not say what went on on deck at the time; I looked on the shore.

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552. It is no part of my duty to press you on any point that you do not choose to give information upon, but I was under the impression that you had distinctly expressed the opinion that there were more people than usual on board, and that there was a large number there; but as you say that you have not been on board a launch for many years, I don't wish to ask you any more questions on that.—What caused me to look ashore was that I had a son there, a boy,

and it was to watch for him that took my attention away from the deck after she went off.

553. I only thought that as an intelligent man would be able to repeat here, what I understood thought, that there was a large number of people on the spar deck of the vessel, and that it was crowded, and very unusual?—I don't think that before.

Very well, I will not trouble you with any further questions.

A. McLean.

ALLAN McLEAN, examined.

554. (*The Commissioner.*) You are a rigger?—Yes.

555. Where were you at the time of the launch?—Amidships.

556. On the spar deck?—Yes.

557. How many men do you think were on that deck?—I did not count the people; there would be about 100 men round me.

558. What did you see in the way of loose materials on the deck? Did you see any chains?—Yes; I saw one chain; I think it was on the starboard side.

559. What was that chain?—I suppose it would be a mooring chain.

560. Was it merely one chain lying along?—Yes. It was ranged fore and aft.

561. Did you see any mass of chain lying anywhere? No, not much.

562. Nothing that struck you as being wrong at the time?—Well, no.

563. Have you not stated that you thought the ship was top-heavy?—I could not account for the top-heaviness, because I did not know. When I went home I mentioned to my wife about it. She asked me if there could be anything wrong. I said I did not know, unless it was top-heaviness. That was all I said to her.

564. That was a very reasonable suggestion to occur to your mind, apart from anything you saw, because I suppose the universal idea would be that anything that would capsize would be, under the conditions, top-heavy. Was that all you said?—That was all my idea.

565. You do not mean to say that the view of the deck, as things lay about it, caused that impression before she careened?—There was some loose material, such as wood and things like that, knocking about.

566. Were you obliged to be on board the vessel at the time of the launch?—I was sent down from Mr. Wilson, the master rigger, to take the boat up to the quay, and get some stuff from the store to work with it at the quay here.

567. Did that require you to be on board at the time of the launch?—Very often there are four or five of us sent down for that purpose.

568. But on that occasion were you obliged to be on board to do that?—Yes.

569. Had you any fear or apprehension at the time of the launch?—No. She went beautifully off the ways until she reached the water.

570. But that is not the question. The question had you any fear or apprehension from what you saw of the ship before the accident that she was top-heavy or likely to capsize?—No.

571. You formed no impression of that kind?—No.

572. And you never told anybody that you thought so?—No; not so far as I mind.

573. What about the number of men?—I daresay there would be about 100. I could not say because I did not think of such a thing at the time.

574. Were you not alarmed before the launch saw so many people so high up on the ship?—No, I was not. There were a good number on the upper deck who jumped with me into the water.

575. But were you alarmed before the accident to any degree?—No.

576. Not at all?—No.

577. You had no fear of anything?—No, she took the first cant, and then she tried to right herself.

578. You think she did?—She did a little came up a little, but on the second cant she went over. I was standing amidships like, made a grab at the weather rail; that is an iron rail that goes round forward, and I went clean overboard.

579. On the port side?—On the starboard side.

580. What made you go over it?—I was right just to the driest part of it.

581. Did you observe anything as to any openings in the side being closed?—No.

582. You did not notice either of the large port or starboard doors open?—No.

(*The Commissioner.*) I think I shall content myself with thanking the firm for having facilitated the coming of these witnesses; but, on looking down the notes that I had prepared to the best of my ability beforehand, I don't think there is any calling any more of the 12 witnesses that I gave the names of, because they seem unable to give information. I mean to say that those who have not added materially to our information do not see any use in accumulating evidence of that kind. I shall be very willing now to hear any witnesses whom you may be disposed to offer.

(*Professor Robertson.*) I purpose to bring you a gentleman distinguished in his profession as a naval architect. I mean Mr. Elgar of London, who has been brought down to give Messrs. Stephenson the benefit of his experience and advice in a matter of much seriousness and difficulty.

Mr. F. Elgar.

MR. FRANCIS ELGAR, examined.

583. (*Professor Robertson.*) You are a consulting engineer and naval architect in London?—Yes.

584. I think you are a Fellow of the Royal School of Naval Architecture and Marine Engineering?—I am.

585. Are you also a Member of Council of the Institution of Naval Architects in London?—Yes.

586. Were you requested by Messrs. Stephenson to investigate, as an independent witness, the causes of the accident to the "Daphne"?—I was.

587. Have Messrs. Stephenson furnished to you every possible information in the way of assisting you towards investigating these causes?—Yes; everything.

588. Will you be kind enough to state your opinion to Sir Edward? Have you prepared any paper on the subject?—I have results which I shall be glad to give to Sir Edward. I should wish to give in the form in which Sir Edward would prefer them.

589. (*The Commissioner.*) As I think I am unacquainted, except conjecturally, with the details of your evidence, the best way will be to state your own way.—I should like to give you a general particulars of the stability of the vessel.

590. Perhaps you would say generally the points you intend to speak?—I intend to speak on these points,—as to the nearest estimate the

possible to make of the actual stability of the vessel at the time of the accident, and also to attempt to account in some way for the fact of the vessel capsizing. The draught at the time of the launch, as near as it can be estimated, was 5 feet $1\frac{1}{2}$ inches forward, and 8 feet $10\frac{1}{2}$ inches aft, giving a mean draught of 7 feet, and a displacement of 401 tons.

591. Would you kindly explain whether that premises that the whole of the launch got clear of the ways?—It does; if it did not get clear I should assume that the weight of the launch would almost equal its displacement, and would not materially affect the draught of the ship.

592. Just so; only I would suggest that much of the stopping up material is light material, and while it would help the stability if left on the port side at the commencement of the inclination, it might aggravate matters later on?—Certainly. The centre of buoyancy was 3.67 feet above the top of keel. All the figures that I shall give you will be above the top of keel. The metacentre was 7.75 feet above the centre of buoyancy, giving a height of metacentre above top of keel of 11.42 feet. The centre of gravity is estimated as having been 10.76 feet above the top of keel, giving a metacentric height of 0.66 feet, which would be nearly equal to 8 inches. I should like to say something with regard to the height of the centre of gravity, Sir Edward, because, as you are aware, the stability of a ship depends to a very large extent upon that.

593. I should like to ask you at this stage whether that centre of gravity includes all the weights on board of her as set forth in statement No. 13?—It includes the weights set forth in that statement. It is also the position of centre of gravity that is arrived at by the distribution of the weights that was handed to you this morning by Mr. Alexander Stephen. You will find at the foot of that statement that it is given as the result of calculation.

594. That you take as the centre of gravity of the ship?—Yes. The statement gives the details by which that height has been arrived at.

595. Does this assume the ship completed?—No, it does not. It assumes her to be in the condition in which she was at the time of the launch, and with those weights on board that you have the statement of.

596. Those weights are included in this?—Yes, they are included in that. I can show them to you. This (*showing in statement*) is the item of miscellaneous weights on board not secured. I should like also to say with reference to that paper that it assumes the men on board to have been 200 in number. We were obliged to make some assumption about them, and that was the best we could make of it, and we looked upon it as an outside one. We have taken it as the men with their tools, because, of course, the men had tools on board. We have taken 16 $\frac{1}{2}$ to the ton, which gives a weight for the 200 men and tools of 12 tons. Of course there were a great many boys among these. The weight of the men would not amount to that, but something has been put on for the tools.

597. Then you have taken 8 tons on the bridge deck, and 4 tons below?—We had to decide how we would distribute them, and so far as we could rather we might assume about two-thirds to have been on the bridge deck, and we have allowed therefore two-thirds of the 12 tons, that would be 8 tons, having been at a height of about 2 $\frac{1}{2}$ feet above the bridge deck, and the remaining 4 tons are supposed to have been at work below, many on the main deck and some in the hold. I have taken the height pretty high for that 4 tons. I have taken the common centre of gravity of the whole of the men below as being a little under the main deck. I should like to say with reference to that, that if we were to assume that the whole of these 200 men, with the whole of their tools were on the bridge deck it would raise the common centre of gravity of the ship about 1 $\frac{1}{2}$ inch above the figure I have given, but still leave the ship

with a metacentric height of about 6 inches. The difficulty is in attempting to account for a ship with 6 inches of metacentric height and with such a very high side out of the water capsizing at all. One can understand from the ordinary notions about stability that a ship on launching might easily oscillate, but one cannot with the information that has been published up to the present time on the subject, easily understand a ship, even with no metacentric height at all, capsizing. But, of course, our opinions are formed by the investigations that have been made down to the present time on the subject, and the investigations of complete curves of stability have been confined, so far as I am aware, entirely to deeper draughts than launching draughts. I have had a great deal to do myself with the calculations of stability of ships under various conditions, but it has never yet occurred to anybody, so far as I am aware, to calculate the actual curve of stability of a ship at her launching draught. One is generally content to assume that if a ship has some metacentric height she is perfectly safe against capsizing; and the general belief in the profession on the subject is that if a high-sided ship has any metacentric height at all she will have a large range of stability, and her stability will go on increasing until what forms the upper deck of the ship becomes immersed to some extent. For the present purpose, we will assume the main deck of this ship to have been the upper deck, and we will treat the upper works as not contributing to the buoyancy at all.

598. In the use of the term "capsize" in your previous statement, you, of course, imply its strict meaning that the vessel went completely over. You don't mean to say that the difficulty is in understanding that a ship may incline in the launching condition to a considerable angle, but it is that she should not find enough stability to prevent her from going completely over?—You will understand me, Sir Edward, when I say that I cannot understand her passing an angle where she would lose all righting power, and the tendency would be to go further over. I was saying that, so far as our information down to the present time has gone, it has been to show that a high-sided vessel, with even little or no metacentric height, might have a very large range of stability; and that the stability would go on increasing until the upper edge of the deck became immersed to some extent, while beyond that point she would still retain a great deal of righting power. But when we come to very shallow draughts of water, such as we get in launching, we find a different state of things. There has been such little time to get up the evidence in this case that it has been impossible to construct any curves of stability for the sake of showing what the variation of stability actually was in this vessel at the time of launching, but I think I can illustrate what I mean by reference to a vessel with a square section. If you take a vessel with a square section, such as this box (*showing*), that vessel, of course, if floating at any ordinary water line such as curves of stability are generally calculated for, would have stability which would go on increasing until some distance beyond the point where the edge of the deck would become immersed, and she might be expected to have a considerable righting moment at 90 degrees. One would expect under such conditions a righting moment at 90 degrees; but I think, as regards a square section, we may see this, that if you give a very light draught to the vessel she may float perfectly stable when upright, but she may also do so at an angle of inclination of 90 degrees. It is a proposition thoroughly established, with regard to the stability of floating bodies, that positions of stable and unstable equilibrium alternate. There would therefore be a position of unstable equilibrium between these two. We have a position of stable equilibrium at zero, and one at 90 degrees, and we should get a curve of stability of this character (*shows diagram*). That is 90 degrees (*showing*), and with a certain position of the centre of gravity we might have a curve like that and a similar curve there (*showing*). There would be a

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Mr. F. Elgar. position of unstable equilibrium at 45 degrees, showing that there would be a considerable angle of inclination over which the condition of the vessel would be unstable. What I wish to point out in connection with that is this—of course a ship has not a square section, but the reason I use that illustration is to show that a ship with a very shallow draught of water might have a curve of stability that would tend in that direction.

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599. Approximately to that?—Yes, approximately to that, and that, therefore, the curve of stability that one would expect by one's ordinary knowledge of this subject to be a curve with a very large range might be a very small curve of stability indeed. Of course, it must also be remembered that these are calculations that simply apply to statical conditions, and when the latter become affected by the dynamical conditions of launching, such as the velocity of the ship entering the water, and the virtual reduction of the weight of the ship in falling into the water, and every effect the current may have upon her, the ship may receive such an impulse in going into the water as to cause her, without going very far, to capsize. I mention that, and I have used the illustration for the purpose of showing you, Sir, that what appears to be a very great difficulty in connection with this matter may be explained in some way of that kind. Of course, we are inclined to think that the very fact of a ship having capsized proves that she had no metacentric height, and there has been a case which has occurred on this river that I should like to refer to, simply for the purpose of illustration, as I have used this diagram. There was a case that occurred on this river some time ago, the particulars of which were made known through the press at the time, and it was known that the ship capsized in launching, practically capsized, and, I believe, sank in a somewhat similar manner to what the "Daphne" did.

600. What case was that?—That was the case of the "Hammonia," built by Messrs. J. and G. Thomson. I must say myself from the experience I have had in judging of the stability of ships from the calculations that have been made, I thought it was certain that her capsizing was a proof that that ship had been carelessly launched in an unstable condition without any metacentric height, but Mr. Biles is here, the representative of Messrs. J. and G. Thomson, who has the actual curve of stability of that vessel. They appear to have been as much astonished as Messrs. Stephen were in the case of the "Daphne" at the ship having capsized, so they had the whole matter looked into, and they have the actual curve of stability of that ship, which shows that in the case of the "Hammonia" the ship with a few inches of metacentric height—as was proved by the inclining of the ship—and a very high side and a light draught, in a somewhat similar condition to what the "Daphne" was, did have a curve of stability at the time which approximated to the curve that I have described to you.

601. Do you know if Mr. Biles is ready to put that curve in?—Mr. Biles is willing to put that curve in. I should like to point out, Sir Edward, that the question of the ports being open in the upper works of the vessel would not necessarily have affected the tendency to capsize.

602. Within an angle of how much?—Probably they would not enter the water till after the point of vanishing stability had been passed, so that possibly they might not have any effect upon the righting whatever.

603. You will probably understand that I have laid great stress in my examination of the witnesses upon points which have been suggested as elements of the accident without much reference to the scientific view of the case, and only for the purpose of clearing up the facts. It will be my duty afterwards to place such facts as are proved by the evidence in their proper order, and estimate them at their proper value. I mean you must not infer that I lay stress on the port question in proportion to the part that it has

played in the examination?—I thought it might aid your mind on the matter to point out the probable bearing it has on this question. I should like to explain with reference to the effect of the boiler having been out of the vessel, that if the boiler and the casings and the funnel had been put on board before the ship was launched, it would not have affected the stability much. It would rather have reduced the stability than otherwise. The centre of gravity of the boiler alone is about 8 feet 6 inches above the top of keel, about 2 feet below the centre of gravity, and the addition of the casings and funnel at the upper part would have taken the common centre of gravity rather above the centre of gravity of the vessel.

604. Do you mean that the boiler itself is not very far from being in the neutral position?—It is very near the neutral position. I would further point out, that with the state of things, we say may have been possible in this ship—the small curve of stability, with the metacentric height which she had—there is a difficulty to understand her receiving the original impulse that would cause her to turn over, and I don't think that is a thing that anybody is able to speak definitely to. Of course we know that there are certain things that tend to produce an oscillation in some circumstances when a ship is launched, and one of them is no doubt the strength of the tide, which has something to do with it. A ship is launched suddenly into the current, or rather when I say suddenly, she enters it with a certain speed. She enters into a current that is moving sideways to her, and it takes her time to acquire the direction of that current. Until she does acquire the speed of the current in the direction in which it is flowing, it is quite certain that the action of the current must be to disturb the state of equilibrium of the ship, and to affect her stability; and the current under some conditions, can clearly be seen to give the ship an impulse in the direction of inclining. There is also this to remember, that when a ship enters into the water the fact of her having any speed on her at all, and of her being lowered into the water, affects the statical conditions upon which the curves of stability are calculated, and makes it difficult to state exactly by calculation what is the precise amount of stability that is necessary to cover all the possible conditions that may arise. I should like to say that the impression that this investigation, which I have made for Messrs. Stephen, has made upon my mind, and what I have seen of the curves of stability of the "Hammonia," is that a ship requires much greater margin of stability to make her safe against all possible conditions that may arise in launching than I had any previous conception of, and I certainly believe than shipbuilders generally have any conception of. That is all I wish to say, Sir Edward, but I shall be happy to answer any questions.

605. (*The Commissioner.*) The questions that was intending to put to you are rather anticipated by your last sentences, because if I have understood you evidence aright, it is to the effect, that you find calculations, carried out to the extent that you have been able to carry them in the time, that this vessel had a certain definite amount of stability, taking in consideration all the weights that are known to have been on board of her, and assigning to these weights their due position. So far your evidence, if I understand it aright, seems to place this vessel in a category of the ordinary run of similar ships, but few of which as a rule have any large amount of righting element in their launching trim when they are afloat. If that be so it would follow that launching a ship in the ordinary conditions of a ship of this type, it must be taken as one not likely to have much stability because it is part of the modern usage of shipbuilding, and a very proper usage, to avoid giving her excessive stability, and any ship so launched is in a condition that does not admit with safety of any large changes of circumstances at the time of launching. That is to say, that there are varying conditions

that require care; and I did not understand that your evidence goes to remove this ship into an exceptional state of circumstances as compared with other vessels, but rather points to the necessity of a closer investigation of ships generally in their launching conditions?—That is so.

606. Is that your view?—Yes; my investigations point to this, that ships are often launched running risks that are by no means understood at the time. For instance, I would like to say, with reference to that, that, with our knowledge of the subject down to the present time, one has always believed that a ship with a very high side, and with but little metacentric height, has a large range of stability, but that clearly under certain conditions this is not the case. I have no doubt that many ships are launched under the belief that they have a large range of stability when they have not.

607. Did I understand you to say that, notwithstanding all the attention that I know you have given to the stability of ships, you are unacquainted with any instances in which the stability of a ship as regards her launching condition has been worked out?—I never knew the complete curve of stability to be calculated for this condition. I never found it to be considered necessary, provided one knew that the centre of gravity was below the metacentre.

607A. Coming a little more closely to the case, you have stated, if I understand you aright, that the question of whether the boilers were on board or not was of no great consequence in this case, because the centre of gravity of the boiler itself, when placed in position in the ship, would be almost identical with the centre of gravity of all the rest of the ship?—Yes; it would not be much below it.

608. But if the boiler had been in its place, and all the casings and fittings completed, and the funnel in place, as is often the case, then the stability of the vessel would have been even reduced?—Yes; because the centre of gravity would go up as the ship became immersed, and the metacentre come down somewhat; the metacentric height would, therefore, be somewhat reduced.

609. Would you tell me what amount of pains has been taken within your knowledge in order to secure accuracy and truthfulness in this statement, No. 13—the estimate of materials, other than those worked into the ship, that were on board of her at the time of the launch. Can you throw any light upon the means by which the statement has been prepared?—I only know this about it that it has been prepared by the manager and the foreman at the works, the men that had to do with it: and the purpose it was prepared for was to enable me to look into the matter for Messrs. Stephen's own private information. I did not see the steps they took, of course, for arriving at it.

610. But what I want to elicit from you was whether you received the statement of weights and the description of places, and from the drawings yourself prepared the tabular statement, or whether you received the statement?—I checked the heights; I measured most of the heights. The positions were described to me, and I measured the heights.

611. And this statement shows the total amount of loose weights, as they are called, to be about $8\frac{1}{4}$ tons?—Yes.

612. Have you had time to estimate what would have been the effect upon the stability of the vessel, supposing all these to have been worked up into the hull, and complete in their proper places?—I don't think the position would have been affected, because the material was by its work. The deck stuff, for instance, was lying near where it was to be afterwards laid, and the ironwork in the same way, and the joinerwork in the same way.

613. Well, now, having stated that the calculations have shown in this case that there is a probable position of instability in vessels in the circumstances in which this vessel was that has not been anticipated,

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because investigations have not been directed to the subject before, and having spoken as you have of some impulse which was necessary to overcome the ship's stability and to give inclination, do you or do you not think that points to the great importance of diminishing the amount of moving weight where no such investigations have been made?—I think it clearly does.

614. You think that the outcome of the investigations, so far as you have carried it, goes to show that it is desirable in cases in which the statical condition of stability of ships circumstanced as they generally are when they are being launched is to avoid as far as possible the risk of having any large man-weight on deck, or to have any movable weights on the ship during launching?—Yes.

615. I should like to ask you whether, as your evidence goes to open up a very large and important question, ranging far beyond the individual case, you think it would be compatible with your arrangements with Mr. Stephen, for you to construct a curve of stability in this case, such as you say has been done in the case of the "Hammonia," and submit that, and how long a time it would take?—I could not speak definitely as to the time it would take; a good many days certainly.

616. Do you think that the materials exist for constructing that curve?—Yes; it could be done.

617. From your acquaintance with the case, do you think it would be likely to throw any more light on the general question than that which the "Hammonia" has done?—I don't think it would throw much more light. You would see by it the exact circumstances, of course.

618. I will not put any further questions on that point until I have heard the evidence of Mr. Biles, because, with that curve before us, we shall be able to judge of the necessity for it. Then I don't think I will trouble you any further, Mr. Elgar?—There is one point which I omitted to state, and that is that the statement of weights that are put in appear to me to be quite reasonable weights, and that most of them have been checked independently by me.

619. Are you speaking of the statement we have here?—I am speaking of the weights of the ship that you have there.

620. I should like to ask you another question on that point before you leave. I believe you have had under your consideration, for another purpose, a vessel similar in size and description to this?—I have.

621. Have you seen the drawings, scale of displacement, and other particulars of the "Daphne" that have been furnished by Messrs. Stephen?—Yes, and I have used them.

622. And do they agree generally without any serious departure with the particulars of similar vessels that you have been acquainted with?—Yes; they agree.

623. Do you think there is anything in these statements to suggest that if you were to search into them you would discover anything that does not lie upon the face of them. I mean to say is there any reason (because I am quite certain, from my knowledge of you, that you would not give evidence in such a case which you did not agree with), but do you see any reason for supposing that there are, or were, elements in this vessel that ought to be disclosed in an inquiry of this kind?—No; none whatever. I don't think there is anything which has not been disclosed; I don't know of anything.

624. And you have no reason for thinking that there is anything?—No.

625. So far as you have gone, have you found any difference in the stability of this ship and the nearest ship of her size and type that you have had to do with?—I have made detailed calculations of a very similar ship to this, and have used them for the purpose of checking these calculations, and they confirm entirely the figures that have been put before you.

626. Was the ship very similar to this in size and type which you have calculated in fulness and detail,

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Mr. F. Elgar. a ship successfully launched, or did any question arise at her launch?—No; she was successfully launched.

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Mr. J. H. Biles.

Mr. JOHN HARVARD BILES examined.

629. (*Professor Robertson.*) What are you by profession?—A naval architect.

630. Where did you receive your professional education?—In the Royal Dockyard at Portsmouth and at the Naval College at Greenwich.

631. You were connected with the Admiralty for some time?—For about 10 years altogether.

632. Are you a Member of the Institute of Naval Architects?—Yes.

623. Are you in the employment of any shipbuilding firm at present?—Yes; of Messrs. J. and G. Thomson, of Clydebank.

634. In what capacity?—As naval architect.

635. You have had, I presume, a good deal of experience in connexion with testing the stability of ships?—Yes, a considerable amount.

636. Have you read any papers on that subject before the Institute of Naval Architects?—I have read two papers which bear upon the subject of the stability of ships.

637. You have heard the evidence of Mr. Elgar?—Yes.

638. Do you concur in the various results given by him?—In the evidence generally, as far as I have been able to follow it, I quite concur in the results. These appear to me to be quite reasonable, and just what I should have expected.

639. I think you also have had some experience in the launching of ships?—I have seen a great many ships launched. I never had the actual responsibility of launching a ship.

640. But to the firm in whose employment you are at present you are responsible for the stability of the ships built and launched by them?—I am responsible for the stability of the ships in every condition.

641. And for every description of ship?—For every description of ship built by them.

642. You are aware of an accident that happened some eight or ten months ago to a ship built by the Messrs. Thomson, called the "Hammonia"?—Yes.

643. Will you state what happened to that vessel in the course of launching?—The vessel was quite successfully launched, but immediately on reaching the water she took a heel to starboard, and settled down on the starboard side. When she had arrived at the extreme of the heel she commenced to return, and returned for some 10 degrees or so; then she settled down again and lifted again a little bit, and finally settled after a third roll at a heel of from 40 to 50 degrees. In that condition she was towed into the dock alongside the yard, and she gradually settled down on account of a few holes that were in her. She settled down on her side in the dock.

644. Have you formed any opinion as to the cause of that accident to the "Hammonia"?—On the face of it there did not appear to be any reason why the ship should turn over. I very carefully calculated her metacentric height before she was launched, and found, judging by that standard, that she would have as much stability as ships of her size that we had launched before had on launching, and these ships had been successfully launched. She had not a large amount of stability, and it was as little as we would have cared under the circumstances to have launched the ship with, but we were quite satisfied that the ship had stability enough, because ships had been previously launched with the same amount of stability. I may say that as soon as we got the ship on her feet we inclined her and found the exact position of her centre of gravity, and then her curve of stability was

628. I suppose you could not furnish me with any particulars of the condition of that ship when launched as to weights on board, machinery, and so forth?—No, I don't know them.

carefully calculated and the result was rather surprising. This (*producing a tracing*) is a copy of the result, which I hand in to you.

645. (*The Commissioner.*) It would be convenient to put upon the notes if you will describe (what is perfectly clear to me) what the curve shows?—This tracing shows the curve of stability of the S.S. "Hammonia," calculated under the condition in which she was actually launched. The particulars that are necessary for identifying the exact condition are on the curve. The curve shows that the stability reached a maximum at about 33 degrees, vanished at about 53 degrees, and became again positive at about 90 degrees. So far the results of the inclining experiment showed that the estimate of stability had been a fair one, and this result was the surprising part of the investigation. The only reason that we could assign for the accident was that the intrusion of a large ship into a narrow river so considerably disarranged the current which was flowing up that the normal conditions of statical stability were not at all acting at that time, and that there was something going on then of which we had no knowledge, and which we had given no consideration to at all; and, therefore, the only thing we could say was that it was not safe to launch a ship with so little stability as that; but until we had that experience, we should certainly have said that the ship was quite safe to launch with any positive stability at all, and we had confirmation of that by having launched previous ships with as little stability.

646. (*Professor Robertson.*) Would you mention what previous ship or ships you had launched?—There was a ship called the "Catalonia," built for the Cunard Company by the same firm, which, after launching, was inclined, and the stability of that ship was found to be practically the same as that of the "Hammonia," about six-tenths of a foot of metacentric height.

647. But she was successfully launched?—Yes. I was launched in that ship myself, because I had some little doubts about the stability, and wanted to see exactly how the ship would behave. On taking the water she took rather a big roll, but she came up again, and she continued this roll until the energy was expended, when she settled in the upright position, and in a perfectly stable condition. The ship had nothing loose on her deck. We had taken the precaution in that case to have nothing that could shift at all on her deck.

647a. (*The Commissioner.*) You are familiar, of course, with the investigations of all the latest investigators who have inquired into the stability of ships?—I think so.

648. For instance, you are acquainted with the whole investigations that were put on record in the transactions of the Institute of Naval Architects by Messrs. John and White?—Yes.

649. And I suppose you are acquainted with all the text books in our language dealing with the question of stability—I mean the modern works?—Yes, as far as I know of them.

650. Have you ever known any instance before this of the "Hammonia" of the stability of a ship in a launching condition being calculated?—No. I knew of no calculation for a complete curve of stability having been made, or even of the necessity of the thing having been suggested.

651. I should like to ask you whether you are of opinion that this case of the "Hammonia," taking it apart altogether from that of the "Daphne," discloses facts in connexion with the stability of ships

in light launching condition that were not anticipated by yourself, or by any of those that you are acquainted with?—I am quite of that opinion.

652. I think I understood you to say that the result of this detailed calculation of the stability of the "Hammonia" at that launching draught throughout successive angles up to 90 degrees was a great surprise to you?—Yes.

653. You expressed the opinion that you were much surprised at the result of these?—Yes. I do not mean to say that the result might not have been anticipated had consideration been given to the question.

654. But it not having been considered, the result had not been anticipated; that is what I understand you to say?—Yes.

655. I understood also that you made some calculations upon that ship (the "Hammonia") before the launch?—Yes.

656. You did not work out this curve before the launch?—No; only the metacentric stability.

657. This curve is based, as I understand, upon the centre of gravity of the ship obtained experimentally after the accident?—Yes.

658. Do you know anything of the case of the "Hammonia;" were you not launched in her?—No.

659. But you were with the firm at the time?—Yes; but I was not in the yard at the time. I was in the South of England on business.

660. Then you cannot give any information as to the persons on board of her at the time?—No.

661. Have you had any opportunity of comparing the results obtained by Mr. Elgar for the "Daphne" with your results, so as to enable you to say whether she had less or more proportionate stability?—I have seen his calculations, and they appear to me to be reasonable, and just what I should have expected from sections of similar ships—not from the "Hammonia," because that is scarcely a guide to the "Daphne," but of similar ships. I think his calculations are, if anything, on the unfavourable side for the ship.

662. You spoke of the changed conditions as regards stability of a vessel that might be brought about by a comparatively sudden passing of a considerable-sized vessel into a tidal river. I suppose that any disturbing effects of that kind might be, or would they not in your opinion be, aggravated by increased velocity in the tide?—They would be such as would grow with the velocity of the tide. The conditions that would cause the upsetting of a vessel would be a heavy under current, heavier than the surface current; and the heavier the current the greater the tendency would be to upset the vessel, I think. Of course, it is difficult to say exactly what would happen in that case, but, so far as one can judge, that appears to be the thing, and I may say that the conditions of launching, as far as tide and position relatively to the direction of the tide are concerned, were the same in the case of the "Hammonia" as in the case of the "Daphne."

663. I intended to ask you that question, only I notice that the "Hammonia," according to your statement, inclined, and eventually turned as far over as she could to starboard?—Yes; she was launched from the opposite side of the river to the "Daphne."

664. That would rather account for her capsizing in the opposite direction?—Yes.

665. So that the same sort of conditions might have been at work, notwithstanding that the vessels capsized in opposite directions?—Yes.

666. I don't know that it comes within the scope of this inquiry, but I might ask you, without any impropriety, whether the inference you draw from your investigation of this question thus far is or is not

that a more general consideration of the stability of ships when launched is desirable?—I think it is probable that rather too little consideration has been given to these questions, but it is a question for each shipbuilder to decide for himself what capabilities he has of investigating these cases.

667. At any rate, your evidence is given to show that the mere fact of a ship being safely launched is not proof that a nearly similar ship being launched under the same conditions will be launched with safety, because, as I understood you to say, the "Catalonia" was somewhat similarly circumstanced to the "Hammonia," and did not capsize or unduly incline, while the "Hammonia" did?—Quite so. As far as we can see of the conditions they were the same, but of course there must have been some difference.

668. Then, I suppose, you would agree with Mr. Elgar in thinking that these investigations at least point to the desirability, where calculations of stability are not made before the launching of a ship, of keeping as much movable weight off the decks as is convenient?—Quite so. We have a general order since the case of the "Hammonia" that no weights are to be on the deck of the ship—no loose weights.

(*Professor Robertson.*) We have no more witnesses here to-day. We proposed to call Mr. Pearce and Mr. Denny, but they are not here at present.

(*The Commissioner.*) Perhaps it is proper to state at this point that I have repeatedly given notice of my readiness to receive any evidence which may come from any quarter tending to throw light upon the causes of this accident to the "Daphne." Hitherto I have not received any proposals to offer evidence from anyone whatever, except the persons to whom I have applied, excluding from that statement the firm of Alexander Stephen and Son, who have from the first volunteered the fullest information and evidence that might be desired. I think it right again to say, in order that there may be no fear of anyone supposing that due notice has not been given, that I shall sit again to-morrow morning at 11 o'clock, and shall be ready then, or shortly afterwards, to take any evidence that may be tendered by any persons tending to throw light on this subject. I don't think it necessary, in the present state of my information, to announce any further sitting of the Commission, although I think, from it being part of my duty to examine the various drawings and statements that have been put in, I shall probably to-morrow adjourn nominally for some little time. I don't at this moment know how long, whether it will be a short time or long, but I shall not be able, unless I receive notice by to-morrow morning, or in the course of to-morrow, of persons desiring to give evidence, to invite further evidence, and I shall not think it necessary to do so. I shall conclude then that there is no one who has any desire to offer evidence upon the subject. I say all that, of course, in order to make it quite clear to the public that, so far as I am concerned, I wish everyone to have the fullest opportunity of contributing to the usefulness of this inquiry. I think, Dr. Robertson, that to-morrow morning we will sit again at 11 o'clock, and then if Mr. Pearce and Mr. Denny, and another witness or two to whom I have written, will come forward, they can be examined, but if such witnesses as Mr. Pearce and Mr. Denny should find it inconvenient to be here at 11 o'clock, we can, of course, adjourn till a later hour, if no one else is present.

(*Professor Robertson.*) Mr. Kemp, in his evidence yesterday, undertook to put in a paper and a section, and he will now do so.

Mr. Kemp put in tracing, showing the disposition of weights and the machinery on board the "Daphne" (marked No. 14), and a statement of weights in connexion therewith (marked No. 15.)

Adjourned.

Mr. J. H.
Biles.

11 July 188:

2 July 1883.

Glasgow, Thursday, 12th July 1883.

(*The Commissioner.*) Professor Robertson, before we commence, I should like to recur for a moment to what was said yesterday about the curve of stability for this vessel. It appears to me, although I should have wished to save the trouble of the calculations if I could have done so, that it is very desirable to have the curve of stability. I think the evidence of Mr. Elgar and that of Mr. Biles could hardly be regarded as satisfactory and complete without that curve. I therefore ask you whether you can, in name of the firm, undertake to produce it, provided I keep the inquiry open long enough for the calculations to be made?

(*Professor Robertson.*) We must, Sir Edward, comply with your request, which, in the circumstances, I must say is a very proper one. The only question is as to the amount of time needed for the purpose. I understand from Mr. Elgar, with whom I have just been communicating on the subject, that he could not promise to let you have it until about the end of next week. It might be gone into much sooner, but it could not be gone into so carefully or so satisfactorily as would otherwise be the case.

(*The Commissioner.*) Well, it would be necessary for me to conduct similar calculations; and as I should not feel justified in closing the inquiry without myself conducting them, and they will take time whoever makes them, I shall be glad to accept your offer that they should be made in the course of next week by Mr. Elgar or by the firm.

(*Professor Robertson.*) By Mr. Elgar, on behalf of the firm.

(*The Commissioner.*) Thank you. There is one other point, Professor Robertson, I wish to mention to you. I should be glad if you could produce some evidence on what is perhaps a minor point, but still not without its importance, touching the distribution of the men on board at the time—that is, whether any bodies have been recovered from the hold of the vessel, or whether it is known that there are bodies at present in the hold of the vessel? When you have had time to consider the matter, if you will be able to furnish any evidence upon either of these points, I should be glad to have it.

(*Professor Robertson.*) Just so, Sir Edward; I shall attend to that. I understand that Mr. Pearce is here in accordance with a special request or summons addressed to him by yourself.

(*The Commissioner.*) Yes. I thought it would be expedient that I should have the advantage of such evidence as Mr. Pearce could offer. From my knowledge of his antecedents, I thought it was highly probable that he would be able to throw independent light upon this question, and I therefore wrote to him, and requested that he would kindly do so. I have just had the advantage of speaking to Mr. Pearce, and I find that he is desirous to have it understood that he is here in consequence of that request. I have not the slightest objection to your putting any question to Mr. Pearce.

(*Professor Robertson.*) I should prefer, Sir Edward, that you would examine him yourself. I have no questions to put to him at this stage. At a later stage I may have some questions to put to him, but not at present.

Mr. WILLIAM PEARCE examined.

Mr.
W. Pearce.

669. (*The Commissioner.*) Then, Mr. Pearce, would you be kind enough to favour me with any suggestions that you have to make?—I am glad, Sir Edward, that you have given me this opportunity of publicly stating that I am not here in response to your advertisement asking those persons who are desirous of giving evidence to come forward. With reference to the evidence that I have to give, my antecedents, I suppose, are so well known that it will not be necessary for me to state them.

669a. They are perfectly known to me.—I may state that from my experience in launching ships, which has extended over a great many years on the Clyde, I have observed that during the last half-hour of the flood tide there has often been a considerable rush of surface water. I know it to be surface water from this fact, that although the current is apparently greater at that time than at any other time of the flood, the rise of water does not come up in the same proportion as it would do in any other time of the flood. From that reason I have always been of opinion that there is a down current below this surface water; and when I attended the scene of the accident at Messrs. Stephen's yard about 20 minutes or half-an-hour after it occurred my first observation was directed to that. I thought the cause of the accident might be attributed to that. I, at that time, did not know any of the other circumstances of the case; but as I had always had that fact upon my mind in launching a ship, and as I had seen some effects, no serious effects, but some effects, of the influence of this current upon the vessels, I naturally came to the conclusion that that had something to do with it. This may have been the first cause, setting other causes into operation. What these other causes are I cannot, of course, positively state, but I may give an opinion upon the causes if you will allow me to do so.

669b. Certainly.—I presume you wish me to do so?

670. I shall be very glad indeed.—The vessel has a great height of centre of gravity, and a compara-

tively small metacentric height. She is naturally what we should call a tender vessel, and liable to influences, and especially liable to the influence that I have just referred to. That influence acting upon the vessel at the surface of the water, and probably the down current at the keel, would give the vessel a list. She would endeavour of course to seek a point of rest, and in that endeavour she careens, because a vessel as she careens gets greater stability, and naturally comes nearer to the point of rest. It may have occurred in this instance that, when the vessel careened, loose material, tools, and men fell to leeward, and a considerable amount of weight went to leeward and prevented her from stopping at what would otherwise have been a point of rest, passing her maximum stability, passing the point where the water could get into the vessel, and the vessel then filling and going down. There may have been other causes operating to bring about the accident. There is one which occurs to me which I don't see has been mentioned at all in any of the evidence, and it is a matter that I have noticed in launching vessels. I have seen them careen, and I have inquired into the cause of that, and found that the bilge ways on one side have still remained under her, and the floating power of these bilge ways has lifted the vessel on that side. I don't say that that is the case here, because I have not heard whether there is any evidence to show that it was the case, but it may have been a contributory cause to careen the vessel. I have nothing further to say at present.

671. I would first take up your last point, Mr. Pearce, with reference to the bilge ways. I suggested yesterday to one of the witnesses that much of the launching gear of a vessel, the moving cradle, bilge ways, stopping up, and so forth, is of comparatively buoyant wood, and would probably exert, and does actually exert, some considerable disturbing force when it remains under the vessel as regards her stability, an item which might be of importance when the vessel is

very light and the stability small; and I suggested whether in a case of this kind, where there are causes at work, whether tidal or other, giving some initial inclination to a ship after she is afloat, this floating material which frequently remains under the vessel would not have this effect, that while, if it happened to remain on the side to which the vessel inclines, as it probably would do, it would in the first instance check the inclination of the vessel, yet nevertheless after she had passed a certain point of inclination it would tend to trip her over? I would like to ask you whether you concur in that view—in other words, whether in the upright position it would not contribute to resist the inclination, while, after she had passed a certain angle, it would not tend to trip her?—That would be the case, Sir Edward. It would be the case immediately that the centre of buoyancy became further out from the middle line of the ship than the bilge ways would be.

672. Mr. Biles was good enough to put in evidence yesterday a curve of stability of the "Hammonia" as launched, and that curve indicates that the maximum stability was reached at something like 31 or 32 degrees, and that the vanishing point was reached at something not very much over 50 degrees. Now, under these conditions, it seems to me highly probable that any launching material that might be under her would, as you have indicated, when the upright action was forced to the wrong side of the centre of gravity, have an upsetting force?—It would.

673. You agree with that?—I do.

674. With regard to your view about there being at times, and within something like half-an-hour of high water, a surface flood up the river, with an under current down the river, have you considered, presuming that state of things to exist, what would be the effect of the upsetting force that would result from that state of things upon a vessel trimmed a good deal by the stern? I may state, for your information, that it has been given in evidence that this vessel, at the time of the launch, was estimated to have drawn 5 feet 1½ inch forward, and 8 feet 10½ inches aft, and therefore to have trimmed by the stern to the extent of about 3 feet 9 inches. In addition to that trimming by the stern, of course there would be a very large increase of stern immersion due to the declivity with which the ship was moving. Have you at all considered how far this upsetting tendency of the contrary currents would be affected in its action upon the ship by that circumstance of the increased immersion aft, or will you yourself consider whether it would add to, or diminish from, the effect of the tide?—From the length of this vessel, and from the position she was in when she was brought up, the stern of the vessel would be directly in mid stream, where the greatest amount of current would exist, and the greater the depth of the vessel the greater this influence would be. Therefore, I should think that the upsetting tendency would be greater if the vessel is by the stern 3 feet 9 inches than if she had been on an even keel.

675. I understand your view to be that that would be so, provided the current were uniform across the river?—Pardon me, Sir Edward, I mean that the vessel's stern would just be about where the greatest amount of current would be, in mid stream.

676. Then you mean to imply by that that you consider that the effect of that force would be all the greater because of the difference of current in mid stream and near the shore?—And near the shore where the stem would be.

677. That is the point I wish to bring out. Now, it is very important in an inquiry of this kind, that any such suggestion as you have made, and coming from a practical gentleman like yourself, should be thoroughly understood, and I would therefore ask you a question or two upon it, not so much for adding to my own information as for bringing out your view clearly. What I understand you to say is, that you have observed, probably not only as a shipbuilder, but as a yachtsman and otherwise, frequently upon the river itself, that there are at times, and particularly

near high water, velocities of the surface water that are out of proportion to the surface rise of the tide at the time: that is to say, that ordinarily with a given velocity there would be a given rise of tide, but that you have observed sometimes, without an increase in the rise of tide, a certain undue and unexpected surface velocity of the stream?—That is so.

678. That is what you mean?—That is so.

679. Now, passing from these considerations, I hope you will excuse me for asking one or two other questions. In launching ships, which frequently occurs, no doubt, at your establishment, have you any means by which you can in any degree influence the traffic of the river at or near the time of the launch? is there any local authority who can give you power to keep the river clear for any length of time whatever?—There is only one local authority that has that power, and that local authority won't exercise it.

680. What is that local authority?—The Clyde Navigation Trustees.

681. They do not give the builders any power to interfere in any way with the traffic of the river?—None whatever. We always send out two boats, but nobody takes any notice of them. They go up and down just as they like, irrespective of boats and irrespective of all warnings, because they have the right of the river before we, who are not in the river, are going to launch our ship.

682. Would you tell me whether that has any practical effect upon the times of your launching? If you had the power to cause vessels to stop at the actual time for launching, would you thereby be able to launch nearer the top of high water than you are now able to do, seeing you have to take the chances of the river being clear?—If we had matters under our full control we would certainly launch very near the top of high water, but we necessarily take the precaution to launch from 20 minutes to half-an-hour before high water, because we might be detained as long as that at a very critical stage of the launching.

683. You were for many years in Her Majesty's service in Chatham Dockyard?—I was.

684. And you were familiar with the circumstances of launching Her Majesty's ships in the royal dockyards?—I was.

685. Can you say from that knowledge whether they usually select almost the top of the tide for the time of launching?—Always the top of the tide, if possible. If anything prevents it being at the top of the tide, it is something that is occurring in the process of launching, not as in our case, where we are influenced by the traffic.

686. Would you give me your opinion as to the effect of this absence of power to interfere with the traffic upon a narrow river like the Clyde, compared with broader rivers, such as many shipbuilding yards are placed upon. It strikes me that it might be a very serious thing that might compel you to launch, or be ready to launch, earlier than if the river were broader, and you had more scope; is that so, do you think?—If the river were broader, of course vessels would pass the vessel that is to be launched very much further away, and have a very much better opportunity of escaping in case the vessel went off whilst they were passing.

687. Of course, I would not have you to understand, in putting this question, that I am expressing any sort of opinion as to the propriety or otherwise of the Clyde Trust regulations. I am wishing to elicit the facts; because I can well understand that a new class of difficulties might come in, of a serious nature, by the Clyde Trust giving complete control to the many private builders along the banks of the river?—I wish you would express your opinion upon that point pretty loudly, Sir Edward, so that the Clyde Trust might act differently to us.

688. I may do so at a latter period possibly, but I do not wish to be understood as conveying any indication at present. Do you ever launch vessels at Fairfield with their machinery on board?—No. We

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have not the means of putting the machinery on board before the vessels are launched.

689. Do you never put even the shafting and propellers in?—Always the shafting and propellers, but not the machinery and boilers generally.

690. Do you exercise any restraint in your establishment as to the number of persons allowed on board ships at launching?—Yes. I make it a point to prevent anybody being on board except those who are actually engaged in the launch, unless permission is asked.

691. You need not answer any questions of this sort which you may find it inconvenient to answer, or which you may not be able to answer, but I am sure you will excuse me for putting any questions of the kind. Do you find any tendency in men who are not engaged on the vessel, and not even employed in the yard, but who may have been employed there, and whose faces may be familiar to the officers of the yard and the establishment—do you find any tendency in such men to get on board during the launches?—It is constantly the case.

692. So that there must be some difficulty in restricting, with anything like rigidity, the number of persons who make their way on board?—We almost always send men off the ship who should not be on board during the launch.

693. I do not like to put any questions to you as a scientific shipbuilder on points which you have not had a fair opportunity of considering, but still your constant familiarity with these questions will leave you at a very small disadvantage. Would you kindly take that curve of stability of the "Hammonia"? (*Hands the witness the tracing, No. 13*). It has been stated in evidence that such a curve as that, or even the elements of a curve which determines the stability at different angles, has not been habitually, or even at all, made for merchant steamers. I should like to ask you in the first place whether you have not in your practice, in launching war vessels for instance with armour plates upon them, made calculations of the stability of the vessel, of more than the metacentric stability—I mean of the stability at successive angles of inclination—of the stability of such vessels at the time of launching?—I have got out the stability curve at the launching draught for war vessels, and I have done so for merchant vessels. I very seldom do that for merchant vessels, but when armour plates are upon war vessels I always do so.

694. Making allowance for the marked differences between war vessels and vessels of this type, and having in view all that you have had done in that way, do you see anything improbable, anything that you would regard as very unexpected, in the character of that curve for a ship in the condition in which that vessel was, she being very light?—The righting lever of the curve is short, and she arrives at the maximum amount of stability at a comparatively small angle, but she has a stability up to a large angle, and the effect of this curve would show that the vessel would incline quickly at first and gradually become slower, and then go on quicker again after passing the angle of about 31 to 33 degrees.

695. That curve, as you see, represents the statical stability of the ship at the different angles of inclination?—Yes, I see that.

696. Does it not appear to you very desirable, having regard to the dynamical stability that that would indicate, to avoid the possibility of any considerable weights being allowed to shift on board a vessel so circumstanced during the time of launching?—If a weight is shifted on board a vessel having

that curve of stability, the curve becomes altered, because the circumstances have altered.

697. If that curve represents approximately the condition of merchant vessels—of course, I am aware that there are very large differences in the character of merchant vessels, but if that represents approximately the condition of any considerable number of vessels, do you not think it points to the desirability of keeping anything like a large number of men out of a ship during the launch?—Certainly it does; and also the desirability of keeping weights as low as possible.

698. And more or less lashed?—And more lashed.

699. Do you consider that there is a particular disadvantage in having bodies of men on board; because they represent weights which they are able to remove themselves to the highest point, and will do so; and, secondly, because they are easily shifted of all weights? Do you concur in that view?—I quite concur with it.

700. And taking this case that has occurred, passing far beyond it to the general fact that there are a large number of vessels that are designed to be built without any large stability, even on sea conditions, does it not point to some general danger in your opinion resulting from that, or that would result from any large admission of persons to vessels at the time of launching?—The present case is the evidence of the danger.

701. You stated that when you launch your vessels they have not usually the boilers and engines on board, because you have not conveniences for putting them on board before the vessels are launched; you remove the vessels to any distance after launch to receive the machinery, or do you take it on near your works?—We have a basin in our yard, and the vessel, immediately after being launched, is placed in the basin to get machinery and boilers.

702. Then that makes it unnecessary, I presume for you to place on board your vessel when launched all the materials intended for her, because she is remaining at your works?—Yes.

703. So that you would not, as a rule, in your establishment, have much loose material on board a ship? You would not have put it on board for the purpose of having any of it taken in her from the place?—No; on the contrary, we keep everything out of the vessel until after she is launched, but it is more easily got on board with us after launched than when the vessel is on the slip.

704. That state of things is due to the fact that you have a basin at your works?—Quite so.

705. Have you seen the drawings of this vessel? Do you know intimately her character?—I saw a mid-ship section of the ship.

706. You perhaps are not sufficiently familiar with her to express any opinion on her character?—No, I am not.

(*Professor Robertson.*) I have no questions to put to Mr. Pearce. I had intended to put two or three to the state of the tide at the time of launching, you, Sir Edward, have anticipated me with regard to that, and therefore it is unnecessary for me to put more to Mr. Pearce.

(*The Commissioner.*) Then I am very much obliged to you, Mr. Pearce, for your kindness in giving this valuable evidence.

(*Professor Robertson.*) Mr. Mansel is not in attendance. I understand that you, sir, have him to attend, and I see him here now.

Mr.
R. Mansel.

Mr. ROBERT MANSEL examined.

707. (*The Commissioner.*) Having understood, Mr. Mansel, that you were in a position—I mean, not understood from yourself, but from other parties, that you were in a position to view this launch, and happened to have done so from your works, I took

the liberty of asking you to come and see if you could contribute anything to my information upon the subject; and knowing, as I do, that you have long accustomed to observe facts in connexion with naval architecture closely, I am very glad that

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been kind enough to respond to that request. I, instead of putting to you a series of questions, in the first instance you would be kind enough to state generally whether you saw the vessel, what you observed about her?—Do you wish me to give just a sort of narrative of what I saw, Sir Edward?

Yes, if you please?—At the time that this was about to take place my firm were preparing to launch a larger vessel from our yard, which is about 300 yards, I think, below Messrs. Napier's, on the opposite side of the river. I had expected that this launch was to take place at or past 11 o'clock, but that hour came, and there was no word of the tugs that were to have towed the launch up to Broomielaw. My manager, Mr. Alexander, went and mounted an elevated platform to see if there was any sign of these tugs coming on. About half-past 11 or so I went up to him and said I thought we should delay no longer, that it was a calm day, and it was no matter for the tugs to be in attendance. He said to me, "There is no sign yet; see, the tide is running up strongly." This called my attention to the tide still running up the river, and was rather inclined to wait a short time; at that moment he said, "See, there is that launch off from Stephen's." I was not aware that there was to be a launch there until he called my attention to it. I turned round, and from our position looked directly up the line of the "Daphne." We saw the vessel coming off very quickly and ran right into the river all right; but one thing I noticed, she went very deep aft, which, as I am accustomed to launch my vessels without machinery, seemed somewhat peculiar to me. She never rose. She floated to the water nearly to the knuckle—I should say running straight in. Then I directed my attention forward to see how things were going on there, and saw her falling down at the end of the ways, floating clear of the ways. A minute after, not a minute, but a second after, I again looked to the stern, and saw it like begin to swerve over to the port side. That movement continued without intermission, and I did think, and meanwhile I saw the men begin to run over the rails on to the top side. When she was astern on her beam ends, and a number of men were on the top, Mr. Alexander and I ran down the gangways and shouted to two of our small boats which were on the upper side nearest Stephen's, in the hope that they would pull as hard as they could up to the launch. Then there was another boat further up the river that we sent up. There were three boats that we sent up altogether. I lost sight of the launch altogether then, because immediately after it was got up, "She has gone down," so I did not look any longer. That is all that I know of the occurrence. The two upper boats that were sent up were sent to our works at Kelvinhaugh. I sent to them that they had saved any of the men. I was told that the launch boat, the one that had been furthest up, had sent the launch first and had picked four men off the "Daphne" and other 12 men, 16 in the first launch.

She then made other two trips, landing a launch, but how many they could not say. The launch boat managed to pick up about 11. The third launch was too late; it was all over before she got up. That was all that I saw with regard to the occurrence of the launch.

9. Have you ever had occasion to calculate the stability as regards stability of vessels at the time of launching?—Yes, very frequently, when I was in the employment of Messrs. Napier and Sons.

10. Would you tell me to what extent you carried your calculations; whether you calculated merely the metacentric height, or whether you did more?—I did not only calculate the metacentric height. I always adopted, at least very approximately, to arrive at the centre of gravity of the hull as well.

11. Did you ever calculate the stability at more than the initial angle for light vessels?—Yes; I have calculated it for different angles often.

712. Would you kindly look at this curve of stability (*handing the witness No. 13 of process*), which is that of the steamship "Hammonia," that you may have heard proved somewhat unstable in launching?—Yes.

713. Is there anything in that that would conflict in any way with the results of your calculations in former times?—No; I have never had it exactly in this peculiar way, but it is the same thing. I have seen similar results.

714. You know that that is a curve constructed after the manner of a curve which was in the first instance applied to an actual ship, in the case of the "Captain," after her loss?—Yes? I believe so.

715. Did you hear the evidence of Mr. Pearce?—No; I did not.

716. It has been suggested, I think, more than once that it is probable that there is something in the nature of contrary currents at work in the river as high water is approached—viz., a surface current up the river moving with a velocity greater than would be due to the mere rise of the tide, and probably attended by an under current downwards; do you think, so far as your experience or inquiry has gone, that that is a probable suggestion?—It is quite my opinion, Sir Edward.

717. That is your opinion?—Yes, decidedly.

718. Have you ever had any occasion to act upon that opinion in any way?—It was brought to my attention nearly 30 years since on Messrs. Napier launching a vessel called the "Lancefield," from Govan, much in the same position as the "Daphne."

719. Can you give me any information upon that case?—The "Lancefield" was a vessel of 225 feet by 32 by 24. I don't know her registered tonnage, but her builders' was about 1,100. The date was 11th October 1854. On going off the ways, she made a violent lurch to the port side, in consequence of which a quantity of chains and smithwork, lying on the upper deck, plumbers' and joiners' tools and materials, slid to the same side, and prevented her from regaining an upright position, and she finally remained floating with her central longitudinal section at an angle of 41 degrees from the vertical, as shown by this diagram. (*Exhibits diagram.*) This account of it was written in 1854. I happen to have looked into the thing very particularly just in consequence of that; but very little more would have sent her over altogether. I made a number of calculations upon this, which are in the paper I have in my hand, which are of no general interest. From that time I always have been of opinion that this question of currents has influenced launches very greatly; but from the uniform success that has attended our launches on the Clyde, there has never been much attention paid to that, until lately the currents have got more pronounced in character. Since they removed the weir, and the opening of the Queen's Docks, I think the upper current is stronger, and at the turn of the tide the downward current is also more pronounced.

720. Would you mind furnishing me with a copy of this paper, that I may place it upon my notes?—Certainly. It is not complete. There are a lot of calculations in it, but it just happens to have been a spare copy or draft that remained among my papers.

721. Then I ask you to furnish me afterwards, in any form you think best, that information?—I will be most happy to do so.

722. Would you enlarge a little upon your last statement, that you think there may have been some change upon the tidal conditions in consequence of the removal of the weir and the opening of the Queen's Dock?—Unfortunately I have no definite data that I could produce.

723. But in general terms?—In general terms I believe that that is so. I believe the effect has been to increase the tidal scour, and the velocity of these currents which I know must exist. Always the surface current, just at the end of the tide, is accompanied with a down draught at the turn, and I believe that this fine vessel, sitting very much by the stern,

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her water-line was in the upper current driving her up the river, whereas her deep dead-wood was in the down current, so that that exerted a twisting effect upon her, and overset her.

724. Are you circumstanced in your establishment as Messrs. Stephen are, or as Mr. Pearce is, or do you take your vessels far from your works? I think you have said that you take them to the Broomielaw?—I take them to the Broomielaw. I may say that we generally, almost invariably, launch without machinery, so that our vessels are lighter and don't dip so deep.

725. Would you state whether you are in the habit of placing on board the vessel before launching the materials required for her completion, that is, materials entering into the hull, and taking them in the ship to the Broomielaw?—Well, I have always been very careful on that point, and endeavoured to keep all the weights down below that I possibly could; and although there may have been some stuff upon deck, I generally endeavour to keep everything below as far as possible. That particular case of the "Lancefield" called my attention to the necessity of doing so.

726. But my question rather was this, whether you put the material on board and sent it up in the ship? You usually do that, I presume?—No, not a great deal. I find it result as well just to cart it up, because it lumbers the deck so much that I am in the habit of carting most of my stuff up to the quay.

727. And that particular personal experience of your own a number of years ago has impressed your mind with the necessity of keeping the weights as low as possible at a launch?—Yes, always.

728. Do you allow persons to be launched on board?—No. I set my face against it as possible, but it is very difficult to prevent it steal on board in spite of you.

729. I don't think I have any more questions very much obliged to you, Mr. Mansel, kindness in coming. Would you allow me to know whether Mr. Alexander, your manager, is present?—Yes.

(Professor Robertson.) May I put one question to Mr. Mansel?

(The Commissioner.) Yes, certainly.

730. (Professor Robertson.) Of course you are in the way of constructing curves of stability?—Complete curves. I have not gone into that.

731. I mean for draughts other than the launch?—Well, it is generally for the launch draught that I have tried the stability. I have, of course, tried it for two or three other points, but I have not done a complete curve.

(The Commissioner.) I think Mr. Mansel stated that he had not made curves of stability in the launching condition. What he said was, that he had made calculations of stability in the launching condition, and he had pushed them beyond the limitation of the initial angle, calculating them at angles, but he had never constructed a curve of stability.

(Professor Robertson.) Then I am quite satisfied with that.

Mr.
R. Alexander.

MR. ROBERT ALEXANDER examined.

732. (The Commissioner.) You are manager to Mr. Robert Mansel?—Yes.

733. It appears from the evidence of Mr. Mansel that you, as well as himself, witnessed this launch from his works on the other side of the river. Would you kindly state your own impression of what occurred at the time of the launch?—I happened to be on a prominent position at the time the vessel was launched, and I drew Mr. Mansel's attention to it. She came off all right into the water, as far as I could see from the position in which I stood. She listed to port, perhaps about 10 or 12 degrees, and then she fell over to about 45 degrees, and she remained at that, just a momentary thing, like as if she stood for a moment, then she went right over until she was lying with her keel and her mid line in the water, and then she turned the keel up. She perhaps turned to about 30 degrees with the keel up.

734. So that she turned to more than 90 degrees from the upright position?—Yes.

735. Would you let me put a question to you which I forgot to put to Mr. Mansel? I understood him to say that, according to his view from his position, which was your position, the capsizing was without intermission; it was one steady turn over. It has been repeatedly stated by persons witnessing the launch that they thought she made an effort to right herself, and came back to a certain extent; was that so?—She made an attempt, as I say—a momentary stop, as it were.

736. It was more of a stop?—Yes, at a point where the water would be at her rail on the port side. The water would be about the rail, as far as I could see, when she made a sort of a stop, and then she gradually fell over. When the water began, as far as I could see, to get to the hatches, she went quicker, until she turned right up.

737. When she passed the point at which the water got free entrance into the side she went faster?—Yes, then she went faster.

738. You observed a marked difference in the rate of inclination?—Yes.

739. Did it strike you, as it did Mr. Mansel, that the stern went somewhat unusually deep, by comparison with your own experience in launching without

engines, I mean, and that it was rather slow in coming up?—Well, of course she did not recover herself very way until she left the ways, but that was on account of the fineness of her stern, which caused her, down the ways, to go deep by the stern. I have seen vessels with engines and boilers, in doing the same thing before, go down deep, but as soon as they were clear of the ways on the fore-end they went up the stern and down by the bow. That is, in a vessel taking the ground by the bow, she stays on the stern until they get a certain distance on her fore-end.

740. Was the fall on the bow on leaving the ways very marked?—Just the usual thing.

741. Were you in a position to observe roughly the number of people high up on board on the deck?—Of course I could not say the exact number, but from the position I was standing in there to me to be from 60 to 80.

742. On the spar deck?—On the spar deck.

743. Did you see any upon the bridge—that is, a bridge above the spar deck?—I did not see that.

744. The whole affair, from the first move of the ship until she was entirely over, was, I suppose, a matter of a very few seconds?—Well, it was possible to form an idea at once, it went so fast, and my mind was so much occupied with the jumping from the vessel, and sending boats and one thing and another; but, I think, I think that a couple of minutes would do the whole thing.

745. Did you notice any slipping or moving of bodies of men down the deck as she inclined?—I could see them on the quarter-deck like, in the fore part of the vessel, but from the position they were in we could not see where they were as they were for the deck houses.

746. As a matter of fact, of course they must have slipped?—Some of them slipped and some jumped over the stern rail. We could see the stern rail from the position we were in. We could not see forward of the houses.

747. Was your ship launched that day?—Yes, it was launched about 20 minutes after that, or about. About a quarter to 12 we launched.

748. It would appear from Mr. Mansel's evidence that you were rather more desirous than himself of keeping your vessel somewhat longer before launching. Was that feeling entirely due to the absence of the tugs and the hope of their arrival, or was it in any degree due to any objection on your part to launch in so strong a current?—It was on account of the tugs not being there that I was anxious to detain her,

because I was afraid that she might go back on the ways with the currents and wind. They sometimes go to the side, and then there is some trouble to get them off. They sometimes take the ground in the falling tide.

749. It was not because you apprehended any evil?—I had no apprehension of any danger whatever.

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Mr. PETER DENNY, Dumbarton, examined.

Mr. P. Denny

(*The Commissioner.*) I did not trouble you, Mr. Denny, with a letter, because I saw you in the room a day or two since, and understood that you were willing to favour me with some evidence on the subject. I therefore thought it unnecessary to write you, but I at once thank you very much for having come.—I don't know, Sir Edward, that I can do better, to save your own time and that of others, than simply to state that I have heard or read the whole evidence up to date. I have heard the whole to-day, and I heard the greater part yesterday, and read the rest carefully this morning. As Mr. Pearce has gone very fully into anything that I could speak upon, I have little more to do than confirm generally the evidence he gave, except on one or two points. Of course, I know nothing about the Admiralty launching, which Mr. Pearce does. Then we never construct—as Mr. Pearce says he did—curves of stability before we launch. The practice is, after we launch, to incline the ship twice, once immediately after she is launched, and again when she is finished, and upon these we act. Of course, as you are aware, Sir Edward, the trouble and time that would be taken to construct a curve of stability from calculations would be very great, and therefore we adopt the plan that was used for the "Captain" to construct them. The other matter that Mr. Pearce and Mr. Mansel spoke upon, and which I don't know anything of, is with regard to the current. I never heard of it until this accident to the "Daphne." Of course, our building yard is on the Leven, not on the Clyde, and the observations that these gentlemen and others have made upon these currents in the Clyde did not occur to us. I don't know that such a state of things exists in the Leven. It is a very much shallower river, as you are aware, probably not over two-thirds the depth of the Clyde. My firm is in the same position as Messrs. Elder and Co. in regard to having our own basin, and, therefore, while if we had to send a vessel away up a distance of two or three miles to the Broomielaw, it is very probable we would put some quantity of stuff on board, we don't require to do that. I don't know that I can say anything else, but if I can answer any questions, Sir Edward, I shall be very glad to do so.

750. I suppose it would not be fair or proper to infer from your statement that you do not calculate curves of stability before launching, that you have never at any time made any calculations at all upon the stability of vessels before they were launched?—No. We always take the metacentric height, and such things as that, and from our experience of types of ships we can arrive at something like an approximation to the centre of gravity. In saying that we incline every ship twice, I mean every type of ship. We may have, as we often have, two or more ships of precisely the same type.

751. In addition to taking the centre of gravity twice, once after launching, and once in the finished condition, do you go beyond that, and make curves of

stability now?—I do not think that is generally done. I cannot speak upon that. I know that we have the material to do it, and I would suppose that it is likely they would do it, but I cannot say.

752. Your establishment, as you stated, is upon the Leven?—Yes.

753. And your conditions are, therefore, not at all the same as regards launching as those of the firms that have to launch into the strong tidal current of the Clyde?—That is so.

754. Therefore your practice as regards admission of persons on board, or any cognate practices, would not have much bearing upon this case?—The practice of allowing nobody into the ship that should not be there prevails, I think, in every yard as well as our own. Sometimes people get in by asking permission, but in Dumbarton we have not the same population, the same number of people coming about us. Therefore we would not have the same trouble as they have in Glasgow, but we would exercise the same care if the circumstances existed.

755. It was stated by one of the witnesses yesterday that in the yard of Messrs. Stephen, and by Mr. Pearce to-day that in his yard, and, I think, by another witness or two, that the restriction of the number of persons on board rather takes the form of putting people out of the ship before the launch who have no business there than attempting to prevent the free ingress of men before the launch. I suppose that is the same in your establishment?—Yes.

756. Except that you have not the same demand for admission?—We have not the same demand, but I can quite believe that that is the case, that people would steal in an hour or two before the launch.

757. You were not present at the launch of the "Daphne"?—No.

758. Have you visited the ship since?—I visited the yard to examine the ways and other things, and I also saw the ship at high water as far as could be seen of her, and looked into general things connected with the subject. Of course I was perfectly satisfied, as you appear to have stated yesterday, or rather on the first day of the inquiry, that the launch was an entire success, and, therefore, there was no occasion to go further into that.

759. I was to ask you that, having visited the ship and inspected the sliding ways, and the bilge ways, you concur with the opinion that I have expressed a strong inclination to, namely, that not only was there no defect in the launching arrangements of the ship, but that all these parts of the launch were equal to launching a very much larger ship with perfect efficiency; you concur in that?—That was my opinion also, and I quite concur with that.

(*The Commissioner.*) Then I have only to thank you, Mr. Denny, for your attendance. I should like, Mr. Mansel, if you would allow me, to ask you one other question bearing upon this last point.

Mr. ROBERT MANSEL recalled.

Mr.
R. Mansel.

760. (*The Commissioner.*) I am, of course, anxious not to dismiss from my consideration, or to finally make up my mind upon any point without being fully satisfied upon it, and therefore I would ask you whether you would, from your observation of what happened, concur with the last witness in thinking

that, so far as you know, there was no reason whatever to doubt the efficiency of all the launching arrangements, and that the accident was due to causes which operated after the effectual floating of the ship? Was that your opinion?—That is my decided opinion.

Mr. Mansel. 761. And you think, I presume, that no useful light would be thrown upon this disaster by any more inquiry into the actual launching arrangements?—
July 1863. The launch came off quite successfully. The vessel was off the ways.

(The Commissioner.) I have received a notification from a gentleman, Mr. Normand, of Havre, who has offered to express his views upon this subject. If Mr. Normand is now present, I should be glad to hear what he has to say.

Mr. BENJAMIN NORMAND examined.

Mr. Normand. 762. (The Commissioner.) You are a member of a firm of ship-builders in Havre?—I am the eldest son of August Normand.

763. Would you be kind enough to state any opinion which you may have formed respecting this accident?—I would ask you to be kind enough to excuse me if I do not explain myself sufficiently clearly in English. I have practised the construction of ships, or at least I have been actually employed in connexion with that, for 40 years. I have seen ships launched in all parts of the world where ships are built, and I ask you to allow me to state a few considerations which I think are not generally known which apply to this case.

764. I shall say at once, Mr. Normand, that, knowing as I well do the eminence that your firm has long enjoyed in France, I shall be much obliged to you for anything you may state.—It appears to me that the real cause of the accident is not apparent to everybody; that is, that it appears to many an inexplicable problem that a ship which should be, as a matter of certainty, from its similitude to existing vessels, in a safe condition to float and to sail, should not be safe in launching. I must say that the explanation of this difficulty has occurred to me only within the last 24 hours. A ship while she is on the ways before she is launched must be admitted to be perfectly stable, and to have an excessive amount of stability, inasmuch as the conditions of stability as they are considered for shipbuilding do not apply only to ships but to all things which are constructed on a yielding platform. There is a metacentre for a coach. There is a stability which may be measured and investigated in the case of a coach or a locomotive as well as for a ship. A ship then is in a safe condition of stability at the moment she is going to be launched, and that ship, by all calculation, ought to be in a condition to be perfectly stable when she is afloat. Between these two situations, however, I find that there is a transition point at which the vessel is no longer stable and safe. If we were to govern the motion of the ship in its sliding advance by machinery and allow it to glide down at a very slow rate, there is a tendency in the great majority of ships, indeed almost all, to have a point at which the ship would be unstable, and capsize to a certainty.

765. How is it that such a thing does not occur?—Simply because the ship advances most generally at a great speed, and as this instant is not of very long duration, and can only take place for a short period of the advance, it passes over it safely. We come now to the point. I admit that most of the vessels launched have very little stability, that is to say, the distance from the centre of weight to the metacentre is something about 1 foot. When the vessel is in a situation to leave the former direction in which she was built and at which she was launched, when the displacement gradually applies itself to the after part of the vessel, that is to say, to a certain part of the body, and there the displacement acts in combination with the amount of supporting powers the vessel finds yet on the resting-point at the fore part of the vessel; when these two forces are equal to the weight of the vessel, then the vessel detaches itself from the ways, and in this particular moment she is borne, not by the force of displacement acting in the centre of the part immersed in the water, but she is borne on a line drawn from that point to the heel of the stem. I do not know if I express myself well in saying the heel; there is another word which I cannot find now.

766. The fore foot?—Yes, the fore foot. If we draw a line from this point, which is a material point, and not an imaginary point as the centre of gravity,

but an absolute and material point, if we draw a line from this point to the centre of gravity of that portion of the displacement which is actually acting to lift the vessel in the vertical, we have the actual either positive or negative stability of the ship, and I contend that in most cases, even in the case of the most satisfactory launches, where no perceptible tendency has been discovered of yielding to one point or another, at this moment the actual weights are above this line upon which the whole system is able to work.

767. You mean the axis?—Yes. If we turn the problem in another way, and instead of launching the ship, if we consider the manner in which a ship should be lifted or hauled on to the ways again, we consider in that case the hauling would be impossible, because when we are simply dragging the vessel from the water in which it enjoyed a positive and sufficient stability, by lifting the fore foot gradually out of the water in that situation, all positive stability will pretty likely disappear, and there will be a point where, the ship not being yet borne on the ways, she will certainly heel over, and that explains why, when ships are to be hauled up, they are generally ballasted first to bring them into a situation in the water more in conformity with the direction of the ways, but very likely not outside of that, and that has a very beneficial and important action in giving the vessel an absolute stability which is necessary for the critical point of transition. It seems to me, then, that all that has taken place in the launching of the "Daphne" can now, and only now, be explained satisfactorily. There was a sufficient amount of stability for the ship to float safely on the river, but I may mention this, that in this point of leaping which truly justifies the word "launch" everywhere applied to the bringing of a ship built into the water, there is something rather critical. It does happen in some places, where we launch the ships on the keel, that this inclining action to the one side or the other may take place. In this country you launch the ship on two ways, one on each side, and a more complex situation arises inevitably. In all the cases that I have seen the upper end of the inclined plane upon which the ship is launched, that is the bearing of the ship on the ways, is not of an absolutely strong and very resisting nature. They are bearings which are intended to yield, and they can yield not exactly the same amount on each side. There may be a side on which more resistance may be afforded, and this will create a tendency or force which may determine the inclining motion; that is to say, the communication of an impact to the whole system. I may also add another thing which refers to the stability of ships at sea. It has been found lately, that is within a few years, that in ships possessed of a very strong steering power, the sudden action of the rudder on the ship going at full speed creates a change, the result of which I will state at once as a violent inclination of the ship towards the concave part of the line that she is following, that is to say, she will incline herself towards the centre point round which she turns. It can be easily explained. The lateral motion of the ship in the water, which is not perpendicular to the length, but is a compound motion, that is an oblique motion of the vessel in the water, naturally causes this action of inclination. In fact, by this lateral motion, there is a kind of metacentre determined aside to the ordinary metacentre, which is the result of the increase of pressure of the water on the side with which the vessel presses the water, and the corresponding diminution of the displacement capacity on the other side. I may briefly point out another instance of that. I lately



built a rapid boat to ply on the Rhone. In the curves of the Rhone, when the vessel possessed a speed which was the result of the sum of the vessel's own speed with the current, that was running 10 metres per second, or nearly 20 knots actual speed, this motion, inclined towards the centre of the curve of the arc through which the vessel was travelling, was very marked. I ask you now, Sir Edward, to allow me to state one or two facts in connexion with certain things which you have been examining about just now. In all countries of the world where I have seen launches, the yards are effectually and traditionally in possession of the public. I have seen people allowed to go on board vessels that were to be launched, and these people thought that they might be of some service sometimes. It may occur that the ship will stick and will not go away, and in many cases I have seen the people on board being asked, or volunteering, to shake the vessel to determine her course. But the practice of having the yards open to the public is a kind of public right on the days of launches. I shall be quite willing to answer any questions you may put to me.

768. I think, Mr. Normand, I have entirely followed your view of the matter, which is to the effect, if I mistake not, that the ship, in passing from the position of stability which she has upon the ways to the position of stability which she will ultimately occupy when launched in the water, passes through a position in which the stability is diminished, because she is partly acquiring stability from the water at the stern, and losing stability from the ways, and that she then passes through a critical condition which may involve capsizing. But then the difficulty that I have in applying that to this case is, that this vessel, according to the evidence, seems to have been launched with the usual speed, so that she was carried past the critical position as well as other vessels; and, secondly, that it is to my mind quite clear in evidence that she was actually afloat at the time, and in the enjoyment of such stability as she possessed when afloat, when the capsizing commenced. Now, would you throw any light upon that subject?—I think, Sir Edward, in launching ships on many occasions we are bordering very close upon danger. I think that the result of a certain number of relative insufficient causes acting together at a certain time will bring that sum to the required amount necessary to cause a disaster. What I want to point out in this is that the circumstances upon which I am going now are not certainly unusual, but I think they have not been put forward anywhere. I think I have read most things that have been written on shipbuilding, and I have not seen them anywhere, and I come to this that many slight causes will concur to bring about such an accident. There is a small thing which may have acted and which might act in other cases to a certain extent, that is water in the ship. The ship has been built under the open sky, and a certain amount of water may have lain in her hold. There is sometimes water brought into the ship. I do not mean to say that there was any in this case, but there might be, and that small amount of water might interfere with the general conditions of the stability in a bad sense. I am afraid to go more completely into the explanations which have been offered. I think the action of the current was such as to cause the lurching inclination in the way it took place. The vessel was launched with her stern towards the water, and the current was flowing up to a small amount at the time. I may say that in all countries it is nearly the rule that we should launch a ship only with a certain amount of current always running. I have always seen ships launched with a little current one way or another, and I think that the unequal action of the current may have caused a little tendency in the ship to lurch when she was passing over this critical space, and may have determined the beginning of the lurch.

769. I think, Mr. Normand, the point of value and applicability of your view to this case lies here, that the vessel undoubtedly lurched; that there must have been causes for the lurching; that one of the causes,

and perhaps a powerful cause in this case, as probably in a very large number of cases, was that during the launching she does pass through a phase of instability as you mentioned, and that during that phase of instability there is a disposition to roll impressed upon the ship. Now, it is just there that value seems to lie in your evidence, and I am much obliged to you for having called attention to it. It is from no sort of desire to detract from the value of your evidence that I say that I don't think you are right in supposing that that point has been altogether lost sight of. I can answer for myself, and say that throughout this inquiry I have had present to my mind the fact that in the transition from the ways to the water there are conditions precisely such as you have described which do tend to make the ship roll more or less, and that is why I have to many witnesses put the question, almost as a matter of course, whether rolling did not usually more or less occur during launching, and that seems to me to give a certain value to your evidence. I would like to ask you one other question, and it is this: You stated that when ships are hauled up on ways they are ballasted in order that they may pass through this transition state?—I did not say that they are ballasted for that purpose. They are ballasted for us to bring them on in a more convenient state, but I think the ballast in them has a very great effect in helping them over.

770. But your point is that the ballasting does accomplish the object of passing them safely through the critical position in hauling them up?—Yes, certainly.

770a. Then your inference would be, at least I presume it would be a fair inference to draw from your view, that were vessels sufficiently ballasted during launching they would pass in perfect safety through that critical period?—Yes. I think that, having got that experience, I should not like to launch any ship without having a certain amount of ballast in the afterpart of the ship. Allow me to insist on that point. The ballast should not be put anywhere, because, on a consideration of the facts that I have adduced, if ballast were put in the fore part of the ship, instead of adding to the stability of the ship it would take away something from it, for the vessel is not in a situation to be inclined along a horizontal line, or a line parallel with the lines of the ship, which are considered with more or less truth to be the basis of motion. I say that it is not the actual basis; that the metacentre is not a point—it is a line, and this line is not necessarily horizontal. The place where the ballast would act most efficiently to complete the necessary stability would be in the after end of the vessel, because there the distance from the centre of weight of this ballast to the axis of inclination would be the greatest. I want you to allow me, Sir Edward, to bring another consideration before you. I have always seen and heard that where ships are launched, even in the most calm waters, and that is evidenced from what you have stated from your experience and knowledge, that there is a critical point of stability in the launching of ships. Old ships were not absolutely in the condition of modern ships in that respect. The ships formerly were built of wood, and were much heavier than they are now. In the passing from wood to iron the conditions of stability have been lessened, first, because the total weight of the hull is less, and second, because the accompaniments of the hull, fittings and decks, and all these things have been to a certain degree increased, because you have on deck heavy winches and all kinds of heavy appurtenances, nearly double the weight of what they were formerly. Now, in the passing from a wooden ship to an iron ship, naturally the ordinary conditions of the stability of a ship have been lessened; and in passing from an iron to a steel ship they have been sustaining another deduction. I beg your pardon for making that observation, but it seems to me to be bordering on the case that we have been examining. Then I pray you to consider that in my opinion all the builders in the world, if they had been asked

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whether this ship, which was to be safe and stable when she was in the water, could be launched with certainty and safety, I think none of them would have answered in the negative, and if I had been at the launch and had been asked to go on board the "Daphne," I would not,—although I am a very cautious man,—have felt the least anxiety about going into her.

(*The Commissioner.*) I am sure, Mr. Normand, it will be very gratifying to all concerned to know that a gentleman of your eminence in your own country

has taken sufficient interest in this case to seek to throw valuable light upon it, and to make such very useful suggestions. I am much obliged to you.

(*The Commissioner.*) We have now arrived at a point in this inquiry at which, in pursuance of what I stated yesterday, I again ask if any one is present who is ready to give evidence upon any point of interest connected with the case? (*No answer.*) I have requested the engineer of the Clyde Trust to be present, and I shall now ask him some questions.

Mr. J. Deas.

Mr. JAMES DEAS examined.

771. You are the engineer of the Clyde Trust?—I am.

772. Will you be kind enough to furnish me with information touching the time of high water on 3rd instant?—I gave Mr. Lamond a statement yesterday, which perhaps I had better read, as it contains the information you want. High water was registered by the Deputy Harbour-Master at Pointhouse Wharf, Govan Ferry, at 11.50 a.m. on the 3rd; height 17 feet 7 inches. Sir William Thompson's self-registering tide gauge at Kelvinhaugh Wharf, Queen's Dock, indicated the same time of tide and the same height of water. I have here the diagram of the latter gauge, and it is of some interest. It shows that low water that morning was at 6 hours 10 minutes, and height 6 feet 8 inches. The total time of rise was 5 hours 40 minutes, total rise, 10 feet 11 inches. The diagram also shows that the tide rose 12 inches between 11 and 11.50 a.m. There were also some questions about distance that Mr. Lamond wished information about. The distance from the "Daphne" to Pointhouse Wharf is 1 mile 1 furlong 86 yards; from the "Daphne" to Kelvinhaugh tide gauge, 1 mile 4 furlongs 113 yards; from the "Daphne" to the centre of Elderslie Rock, 1 mile 5 furlongs 128 yards; and to Renfrew Ferry, 2 miles 1 furlong 33 yards. High water at Renfrew Ferry was ascertained, by actual observation in 1872, as 10 minutes earlier than at Glasgow harbour.

773. How far would that be below Linthouse?—2 miles 1 furlong 33 yards; that is the distance from the "Daphne" to Renfrew Ferry.

774. And the time is what?—10 minutes between Glasgow and Renfrew Ferry. There is only an hour and a minute of difference between Glasgow and Greenock, and the distance is 19 miles 3 furlongs; that is to opposite the Custom House from Pointhouse Wharf. I also made a note of some tidal velocities that we have been taking during the last two or three months at Elderslie Rock. The results are:—

Spring tides, quarter flood, 1 mile 2 furlongs; half flood, 6 furlongs; three-quarter flood, 7 furlongs; quarter ebb, 1 mile 3 furlongs; half ebb, 1 mile 2 furlongs; three-quarter ebb, 1 mile 2 furlongs.

775. I suppose these are surface velocities?—Yes.

776. Am I to understand that the highest surface velocity at flood of spring tides does not exceed 1 mile 2 furlongs?—It does not. It is a very sluggish stream. It is a little quicker now than it was 20 years ago, but still it is a very slow stream except when the river is in flood, and then the ebb is somewhat heavier. I made a series of experiments—it is perhaps not much to the point, but I brought it along with me in case you might wish to have it—of tidal velocities by floods all the way from Glasgow to Greenock, extending over a considerable number of weeks. They were made in obedience to a motion by the Lord Provost of Glasgow.

777. (*The Assessor.*) Was that since or before the removal of the weir?—It is since the removal of the weir. (*The witness read the result of the experiments from the book he put in.*) The result of these observations was to ascertain that while the average level of high water of spring tides has risen in Glasgow

harbour about 9 inches since 1853, the level of high water at neap tides remaining virtually the same, the average low water level of spring tides has fallen about 1 foot 11 inches, and of neap tides about 1 foot 1 inch. Working out the tide gauge readings in Glasgow harbour for 1880 gives the range of spring tides to be now 11 feet 2 inches, and of neap tides 9 feet 2 inches. I hand in a copy of the book for your information, Sir Edward.

778. (*The Commissioner.*) Would you kindly state generally what you regard as the effect of the removal of the weir upon the tidal condition of the river?—I think the result of the observations has been that they have been of a very negative character. In fact, I don't think they have had much to do with the tide at all. One of these tidal observations, from Glasgow to deep water at Greenock, was made in 1879, before the removal of the weir, and you will observe that the flood got somewhat more rapidly towards deep water at Greenock than after the removal of the weir. I think the removal of the weir has certainly been prejudicial to the navigation, in so far that deep-draughted vessels cannot leave Glasgow harbour now so early on the tide by half an hour as they did before the removal of the weir. The crest of the weir was only some 8 inches below high-water level of spring tides, so that it was only the spring tides that got over the weir at all, and the whole of the water that came up was impounded in Glasgow harbour by the weir at neap tides, and only about 8 inches of it got over at high water of spring tides. Now, since the removal of the weir, of course the tide runs away right up through Glasgow, and we don't have the same level of water at the same period of the flood as we had before the removal of the weir; while, at the same time, high water rises as high now ultimately as it did before the removal of the weir.

779. But the duration of high tide is reduced?—The period of obtaining a sufficient depth of water early on the tide is reduced by half an hour, so that the Anchor Line and Allan Line, and other deep-draughted steamers, are about half an hour later in getting away on the tide than they used to be. The removal of the weir has in that way been a prejudice to navigation.

780. Has there ever been made within your knowledge any experiments tending to show the velocity of the Clyde at different depths?—I may explain that those observations that were made in 1881 were made not with a float floating on the surface but with a float formed of a square block of wood about 4 inches square immersed down about 10 feet in the water and loaded at the bottom so as to keep it perpendicular. These floats were watched night and day from their being put in at Glasgow Bridge until they reached the sea below Greenock, so that these tidal observations as to the flow of the tide are not surface observations, but float below.

781. Did that float rise much out of the water?—It was just sufficiently out of the water to be seen from the small boats at night—something like 6 inches.

782. Then its position at any time would indicate whether there was a variable current?—The result was that we did not find it to have any difference. That was really the result of it. I tried some of the

surface floats against the immersed float, and there was no appreciable difference.

783. Did it float upright?—Yes. There is one of them shown in the drawings.

784. It has been stated repeatedly in evidence in reply to questions of mine that no power was given by the Clyde Trust to shipbuilders on the banks of the Clyde to in any way interfere with the traffic on the river at the time of a launch. Is that so?—There is nothing in our rules and regulations regulating the launching of vessels on the Clyde. In 1878 we attempted to get a new set of byelaws and regulations passed by the sheriffs of the three counties through which our jurisdiction extends. One of these regulations was dealing with launches, and I will read it, but in consequence of opposition to it, which, I daresay, Professor Robertson remembers as well as I do, they were not passed by the learned sheriffs.

785. (*The Assessor.*) Whose opposition?—I think it was the opposition of the Caledonian Railway in connexion with their harbour at Bowling.

786. It was not the shipbuilders' opposition?—No; I don't think so. The clause I refer to is No. 32 in the proposed byelaws, and anticipating that I might be asked the question I brought a copy of the rules with me. In the margin it is stated, "Vessels not to pass the vicinity of a launch till same has taken place, or signal withdrawn"; and the clause runs thus: "Every master or other person in charge of any vessel passing or being on the river in the vicinity of any ship launch shall observe the signal exhibited to give notice of such launch, which shall be a red flag with the word 'launch' in large white letters thereon, displayed from a boat on the river, and shall not pass such signal until the launch has taken place or the flag be withdrawn. There are penalties attached to the violation of any of the byelaws; and, of course, any violation of this law, had it passed, would have been attended by the infliction of a penalty.

787. Did this proposed byelaw, No. 32, receive the approval of the Clyde Trust?—Yes; the byelaws were all approved by the Clyde Trust, and after their approval submitted to the three sheriffs, and we were desirous of having them passed.

788. Then there will be no objection, in your opinion, or in that of the Clyde Trust Authorities, to have a clause of that kind?—None whatever. I hand in a copy of the proposed byelaws, and also a copy of the byelaws as they at present exist, dated 23rd April 1860.

789. Before we come to that, I should like to ask another question. I understand that it is not in the power of the Clyde Trust, without the approval of the three sheriffs, to make any change on the byelaws?—No.

790. Do you think it would be in your power, either now or later on, to furnish me, in any form, with the grounds of opposition to the byelaws? I suppose, at any rate, these grounds of opposition were general; they were not to this particular clause?—They were not to this particular clause. Of course the regulations had to be approved of *en bloc*. Professor Robertson was our agent in getting them approved of, and he will perhaps be better able than I am to give you the reasons why the sheriffs did not approve of them.

(*Professor Robertson.*) I am not in a position at present to state why, simply for this reason that the application on behalf of the Clyde Trustees was conducted by a partner of mine, now deceased; but I shall undertake between this and the next diet to produce some kind of evidence or satisfactory explanation of why the wished-for clause was not sanctioned by the three sheriffs.

(*The Commissioner.*) I am much obliged to you, Professor Robertson, for what you say. Of course, the only point on which I would seek to trouble you would be to get, if I could, at the grounds of any objection that may have existed to this particular clause.

(*Professor Robertson.*) Quite so.

(*The Commissioner.*) Because, if there were objections to the proposed new regulations taken *en bloc*, that would not much enlighten us.

(*Professor Robertson.*) I understand your remarks to apply solely to this proposed clause, to which you have been directing the attention of Mr. Deas.

(*The Commissioner.*) Yes, whether there was any opposition to that, and if so, on what grounds.

(*Mr. Deas.*) I think you may take it from me, that there was no opposition to that special clause on the part of anybody. That clause was put in by the Clyde Trustees with the desire of regulating, as far as they thought they would be right to do, the launching of vessels on the Clyde. These by that time had become much more numerous, being of daily, or almost daily, occurrence, than what they were when the last regulations of 1860 were passed. I should, however, desire to say that the trustees did not think they could go further in the direction of regulating them than that clause points out. To divide the responsibility between two parties, one afloat on the river, and the other on shore preparing to launch, would be, in my opinion, a very serious and grave mistake.

791. (*The Commissioner.*) What would have been the limits of the operation of that new byelaw, if it had been passed—I mean the limits on the river?—The limits on the river are not defined. That is left very much, I think, to the opinion of the party taking charge of the launch.

792. But would it apply, for instance, to all the yards between Glasgow and Greenock?—It would apply from Glasgow to Port Glasgow.

793. And on both banks of the river?—On both banks of the river. I should desire, Sir Edward, to refer to a paragraph in one of our daily papers to-day rather reflecting on the conduct of the Clyde Trustees with reference to this most unfortunate occurrence. Far from not giving Messrs. Stephen and Sons every assistance in our power, our only diver has been at work, and I believe he was the first on the spot on the Wednesday afternoon. Punts have been given to assist him in every way, and our harbour-master, on his arrival from London on Thursday, went down to the "Daphne," and offered to place even half-a-dozen of our hopper barges at the disposal of Messrs. Stephen and Sons, for the lifting of the vessel. That offer was declined with thanks by Mr. John Stephen, I think because they had arranged with the Salvage Association for the lifting of the vessel. I desire to make that statement on behalf of the trustees and their officers, of whom I am one.

794. Can you tell me from the information supplied to you by your diver whether any bodies have been recovered from the hold of the vessel, or whether it is known if there are there any still?—I have only seen the divers once since my return from London on Saturday night, namely, on Sunday morning, and I have had no conversation with him on the subject. All that he desired to see me about was to get permission to be absent from duty, as he had been working every tide since the occurrence, and I told him to arrange so as to get the needful rest. There is a plan that I should desire to put in, which I expected to have been here, showing the position of the "Daphne" on the afternoon, and I will wait for a few minutes to hand it in.

(*The Commissioner.*) Having now exhausted all the evidence, Professor Robertson, that has been offered or that I have been able to obtain, and some days of time being required for the completion of the calculation which Mr. Elgar is making for Messrs. Stephen, I myself requiring a considerable time for dealing with the facts before me, and more particularly for making, at any rate, check calculations on some points, it becomes necessary now to adjourn this inquiry. I think it would not be convenient, either to myself or others, to name a time when I can sit again if necessary; but, as far as I at present know, it will not be necessary to sit again except for the pur-

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a time, and to give public notice by advertisement in the papers in Glasgow as before, when I will attend again to sit for the completion of the evidence.

The proceedings were then adjourned.

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(*The Commissioner.*) At the last sitting we adjourned primarily for the purpose of giving time for Mr. Elgar to complete certain calculations on behalf of the builders, and I think the first thing we had better do to-day would be to hear the continuation of his evidence.

(*Professor Robertson.*) Perhaps, Sir Edward, you would allow me to interpose at this stage. At last diet, as you may recollect, Mr. Deas gave in evidence a statement to the effect that certain new regulations proposed by the Clyde Trustees had been refused *en bloc* by the three sheriffs of the counties through which the jurisdiction of the Clyde Trustees extends.

On that occasion you expressed a wish to know why it was that the sheriffs did not approve of the proposed regulations. Unfortunately at the time I was unable to furnish the necessary information, but Mr. Reith, the general manager of the Clyde Trust, is now in attendance, and as he is under the necessity of leaving at an early hour, perhaps you would be kind enough to take him in the first instance. He has prepared a statement which I believe he intends to read to you, and he will be able to state, *inter alia*, why it was that these regulations did not receive the wished-for approval of the three sheriffs.

(*The Commissioner.*) Certainly.

Mr. G. Reith.

Mr. GEORGE REITH examined.

794a. I have been authorised, Sir Edward, to appear here on behalf of the Trustees of the Clyde Navigation, and to put in a statement with reference to a certain part of the evidence which has been already taken in this inquiry. Perhaps you will allow me to read the part that I refer to. It is in the examination of Mr. William Pearce, and it begins at question 679. (*Mr. Reith here read the passage in Mr. Pearce's evidence from question 679 to question 687.*) Now, Sir Edward, I, with your leave, will put in a statement from the Clyde Trustees explaining how matters stand just now, and the origin of the present system. Mr. Reith then read a statement in the following terms :—

“STATEMENT for the TRUSTEES of the CLYDE NAVIGATION with reference to the evidence of Mr. PEARCE, given before SIR EDWARD J. REED, in the inquiry respecting the sunken vessel ‘Daphne.’

“It does not very clearly appear to the Clyde Trustees what bearing the matter of the regulating of launches by signal has upon the present inquiry, and were it not that it has been referred to by Mr. Pearce in his evidence in a manner somewhat reflecting on their action, they would not have troubled Sir Edward on the subject. The Clyde Trustees are quite alive to the importance of the question, and quite anxious that every precaution competent should be taken to prevent accident, but they do not wish to assume a position that might entail upon them serious responsibility by interfering to control the shipbuilders’ arrangements. At the same time, and while the primary use of the Clyde is the accommodation of its enormous shipping traffic, the trustees, recognising the great importance of the shipbuilding interest, have been always anxious to meet the views of the shipbuilders with regard to the launching of new vessels, and to secure safety, in so far as consistent with the rights of the Navigation.

“In a crowded narrow river like the Clyde, the matter can only be satisfactorily adjusted by a mutual understanding between the Clyde Trustees and the shipbuilders. Such an understanding was arrived at several years ago, and is now in operation. It is found to work well, and the trustees are surprised to find Mr. Pearce stating that nobody pays any attention to it. The launching signals are believed to be uniformly observed by the shipping on the river.

“As far back as 1868, when there was no fixed rule in operation with respect to launching, the matter

was brought before the trustees by the shipbuilders, who pointed out the hazard run by reckless persons in charge of vessels in disregarding the signals given at launches, which then was generally a flag displayed at the stern of the vessel. The Clyde Trustees entertained the matter very cordially, and much correspondence took place with the shipbuilders on the subject, but it was found that there was not a sufficient consensus of opinion among themselves to justify the adoption of any new or improved system.

“In 1878 the subject was again brought before the trustees and the result of much correspondence, and a meeting with the shipbuilders was an undertaking by the whole of them, with a few exceptions, to observe certain agreed-on regulations with respect to the kind of signals to be used at launches, and the manner in which they were to be exhibited. On the other hand, the Clyde Trustees agreed to enact a byelaw imposing a penalty on any person in charge of any vessel who should disregard the launching signals.”

The following is a letter from the shipbuilders to the Clyde Trustees :—

“LETTER by SHIPBUILDERS to the TRUSTEES OF THE CLYDE NAVIGATION as to launching of vessels, and relative byelaw.

“TO THE TRUSTEES OF THE CLYDE NAVIGATION.

“Gentlemen,

“As you have, with a view to prevent accident to passing vessels when a launch is about to take place from the banks of the River Clyde, agreed to enact a byelaw imposing on the masters or other persons in charge of such passing vessels the duty of recognising the signals to be exhibited by us respectively to give warning of the launch, we hereby undertake, with concurrence of the Clyde Pilot Board, to exhibit signals, on every occasion of a launch from our respective building yards, of the nature and in the manner following, viz. :—

“1. When a vessel is to be launched into the river, two small boats shall be placed by the owning shipbuilder, at a distance of not less than two hundred yards from the line of launch, the one up and the other down the river; and at five minutes before the launch is to take place, the public pilot in charge of the vessel will see that the river is clear, and will signal to the boats from the stern of the vessel, by waving a red flag of four feet square, with the word ‘launch’ in large white letters thereon; and

on such signal being given, the persons in the boats, appointed for the purpose, will immediately display, by waving, a similar flag attached to a pole, 10 feet long, to stop approaching vessels, and will continue to display the same until the launch has taken place, or the flags be ordered to be withdrawn.

- "2. The pilot will see that the flags are duly displayed from the boats whenever he gives the signal, and if they are not so he will immediately inform the person in charge of the launch of the circumstance. Should anything occur to prevent the launch from taking place after the red flags are displayed, and it has to be delayed or postponed, the pilot, by waving a white flag, will signal to the persons in the boats to withdraw the flags displayed by them, so that vessels may proceed.

"As it is of the utmost importance that the navigation be interrupted for as short a time as possible, we undertake that every effort shall be used by us to get our vessels off the launching ways with the least practicable delay, after the signals on the boats have been exhibited.

"The entire responsibility of the carrying out of this arrangement is to rest with us respectively, and no liability of any kind is to attach to you in connection therewith.

"We further agree to give twenty-four hours' previous notice to the Harbour Master of the day and hour when it is proposed a launch shall take place, and this in addition to any notice that may be incumbent on us by Statute.

"We are,
Gentlemen,
Your obedient Servants."

I may state to you, Sir Edward, that that document was given to us signed by 10 of the shipbuilders on the river. I may as well read the names.

795. What is the date of the letter?—13th January 1879.

796. Would you read the names?—Charles Connell & Co; James & George Thomson; David & William Henderson & Co.; Robert Napier & Sons; London and Glasgow Engineering Company; Alexander Stephen & Sons; Barclay, Curle, & Co.; Napier, Shanks, & Bell; Aitken & Mansel; A. & J. Inglis. The byelaw you have already got a copy of. It is in the following terms:—

"BYELAW.

"Every master or other person in charge of any vessel passing or being on the river in the vicinity of any ship launch shall observe the signal exhibited to give notice of such launch, which shall be a red flag, with the word 'launch' in large white letters thereon, displayed from a boat in the river, and shall not pass such signal until the launch has taken place, or the signal be withdrawn."

"It has been already stated to Sir Edward that the byelaw has not yet been confirmed by the sheriffs, and has not therefore come into operation. But notwithstanding of this, the agreed-on scheme of signalling was adopted, and is now the uniform practice.

"Mr. Pearce was one of the few who did not agree in the arrangement, but he has, nevertheless, carried out the system of signalling adopted.

"The party who took exception to one of the byelaws, and which prevented their confirmation, were the proprietors of the Forth and Clyde Canal. They wished to have one of the byelaws so amended as to make it imperative on all vessels to slacken speed when at 150 yards from the entrance to the canal at Bowling. The trustees declined to concede this right, as it opened up a very important question, whether riparian proprietors and others on the banks of the Clyde have right to control the use of navigation.

"The sheriffs assumed to have power to give effect to the proposed amendment, and make its adoption a condition of their confirming the byelaws as a

whole. The trustees, acting on the advice of their law agents, declined to admit the amended rule, and the sheriffs thereupon refused confirmation.

"The matter was left in the hands of Professor Robertson's firm to advise as to further procedure, but, in consequence of the lengthened illness and subsequent death of Mr. Ross, one of the partners, who had conducted the case, no further step has yet been taken.

"There was no opposition to the byelaw regarding the signalling at launches, and although it has not received the required confirmation, the trustees would wish it to go forth to the public, that any persons disregarding the signals now in operation as the acknowledged signals in use at the launching of vessels on the Clyde, will not certainly escape responsibility from the absence of a special byelaw.

"The trustees are not aware whether Sir Edward intends to express any opinion upon this subject as suggested by Mr. Pearce, but they think it right that he should be put in possession of the whole facts, and that no mis-apprehension should exist derogatory to the trustees as entrusted with the management of a great public undertaking."

I may state in addition to that, Sir Edward, that I happen to be joint secretary to the Clyde Pilot Board, who license the pilots of the river. You are aware that the Clyde is compulsory waters, and that every ship above 60 tons burden must have a licensed pilot on board. That is the reason why the pilot was here taken as the man to regulate the launch, because it was necessary in order to comply with the byelaw that a licensed pilot should be on board the vessel when she went into the river, and he being a public man, it was thought he was the best man to select to give the signals. I may mention further, that so particular are the Clyde Pilot Board members that every man who comes forward to be examined in order to be appointed a pilot, is asked with regards to the signals, first, whether he knows them already, and if he does not, they are then put before him, and he is warned of them, and urged in every way to carry them into effect, so that we have very few exceptions on the river of ships that have not a licensed pilot on board. I am ready, Sir Edward, to any answer further questions, if you think it necessary.

797. (*The Commissioner.*) The only question that occurs to me to ask you is whether you can throw any light upon the grounds of the objection to concur with that letter of the shipbuilders, which objection was felt by Elder & Co. and some other shipbuilding firms. Do you know why they objected?—I think their objection was to the appointing of a pilot. Their idea was that the Clyde Trustees should take the responsibility of sending men to guard the launches themselves. One party, I remember, had no objection but to the pilot alone, because he said, "I can launch my ship and just turn her into my own basin, and I do not require a pilot." Of course that was wrong, because he was liable to have a pilot if he took his vessel into the river at all, but that was the reason he gave. I think there was only another one—three altogether—who did not sign this document. I may mention, Sir Edward, that the proprietors of the Forth and Clyde Canal are the Caledonian Railway.

(*Professor Robertson.*) That is so, and hence their connexion with the harbour of Bowling, which gave them interest to appear in that case.

798. (*The Commissioner.*) I understand that the consent of the sheriffs was withheld on grounds quite apart from this launching question?—Entirely.

799. It was founded on the right of the canal to interfere with the traffic in the river?—Yes; to have a byelaw of that nature.

(*The Commissioner.*) I have looked over your paper, Mr. Reith, because Mr. Pearce is not here, I believe, and I thought there was one passage in it in which you seemed to regard his remark about nobody taking any notice of the signals as applying rather more to the Board than his evidence seemed to indicate. I think he was speaking at that moment more of persons in the steamers, but there is no doubt the general effect

Mr. G. Reith.

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Mr. G. Reith. of his evidence was such as to make it desirable for me to receive this explanation from the Clyde Trustees.

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(*Mr. Reith.*) He wished the Clyde Trustees to do something they were not doing, and he urged you to speak a little plainly on the subject.

Mr. F. Elgar.

Mr. FRANCIS ELGAR recalled.

799a. You asked me, Sir Edward, to put in the curve of stability of the "Daphne" at the launching draught. First of all, I will hand you the curve of displacement calculated for fresh water, and the curves of metacentres and centres of buoyancy [*hands in tracing marked No. 23*]. This [*handing in tracing, marked No. 24*] is the curve of stability constructed for a draught of 7 feet 5 inches, and a metacentric height of $6\frac{1}{2}$ inches. There are two curves there; one is calculated to the top of the main deck, and includes also the poop. The poop has an iron bulkhead across the front, and therefore requires to be taken into account. The other curve is carried to the top of the bulwarks, and shows what the stability would be if all the ports were closed, and the top side plating quite tight at the time.

800. When you say the main deck inclusive of the poop, you mean that the poop is added and the curve carried on?—Yes, because it was enclosed at the fore end by an iron bulkhead. I should say with reference to these curves of stability that the information respecting the total weight of the vessel and her displacement was not fully complete when the curves were asked for and made, and that she has a somewhat greater draught than was then anticipated. The curve requires to be somewhat corrected for that increase of draught, but I have not yet had time to complete this.

801. Are there any points on which you wish to modify, or correct, or enlarge, the evidence you gave previously?—I see no reason for modifying or correcting anything stated in my previous evidence. The inclining experiment, of course, now enables the stability to be determined with a greater degree of accuracy than was possible by the most refined calculations; but you made the experiment yourself, and I do not desire to make any statement respecting it beyond this, that the position of the centre of gravity, I find, agrees closely with that given by me in my former evidence, and the vessel undoubtedly at the time of launching had a few inches of metacentric height. I think that is completely confirmed by the inclining experiment. I only wish to add to what I said before, that, so far as I can see, this disaster, and the accident which happened to the "Hammonia," show that, as I before said, there may be elements of danger in the stability of a vessel when launched which have never been suspected, or at any rate pointed out, up to the present time, and I may go further and say that the necessity of even searching for them had never before suggested itself.

802. Is there anything else you wish to say?—Nothing else.

(*The Commissioner.*) Well, I don't think it is necessary to ask Mr. Elgar to carry his investigations any further for my purposes, because I am now in the possession of the fullest information necessary to determine as exactly as it ever can be determined in this world, I believe, what was the precise stability of the ship on the occasion of the launch, and under the conditions of the launch, and therefore it seems unnecessary for me to ask Mr. Elgar, on behalf of the builders, to carry out independently investigations which I am myself making, and shall soon have completed. I think I quite understood from you, Mr. Elgar, that this curve which you have now handed in is subject to certain corrections. You profess it to be the best you could produce before the experiment was made?—That was the best that the information available enabled me to produce.

803. And you yourself admit that it may require correction as the result of the experiment?—Yes.

(*The Commissioner.*) I am very much obliged to the trustees and to yourself, Mr. Reith, for giving me this information. I don't think I need trouble you with any further questions. Mr. Elgar will now perhaps complete the statement or evidence which he gave.

(*Professor Robertson.*) At the last diet, namely, on 12th inst., you expressed yourself in addressing me in the following terms:—"There is one other point, Professor Robertson, I wish to mention to you. I should be glad if you could produce some evidence of what is perhaps a minor point, but still not without its importance, touching the distribution of the men on board at the time, that is, whether any bodies have been recovered from the hold of the vessel, or whether it is known that there are bodies at present in the hold of the vessel? When you have time to consider the point, if you will be able to furnish any evidence upon either of these points, I should be glad to have it." With reference to the wish then expressed by you, Sir Edward, I have to state that 67 bodies were recovered from the hold of the vessel. That precisely meets your inquiry whether any bodies have been recovered from the vessel, or whether it is known that there are bodies at present in the hold of the vessel.

804. (*The Assessor.*) Has that number been recovered since the last sitting?—No, that is the total number. I think that exactly meets your inquiry, Sir Edward.

(*The Commissioner.*) Yes.

(*Professor Robertson.*) Messrs. Stephen & Sons, I beg to add on their behalf, have made careful inquiry as to the number of persons on board at the launch, and, so far as they have been able to ascertain, this number was 186, of whom 62 were saved and 124 drowned. They are aware that in this inquiry there may be the risk of omission of one or two (more especially of persons outside their works); but as they believe their practice with regard to the exclusion of outsiders on the occasion of a launch is at least as stringent as that of any other builders, they think that any such possible omission must be reduced to very narrow limits; and they would desire to direct attention to the circumstance that among those drowned there was only one stranger, a man who had been taken on board by a workman as a friend. It has been the practice of Messrs. Stephen & Sons—and in this they are not at variance with the other builders on the Clyde—to allow such men as might happen to be at work on a vessel at the time of launching to remain on board; and the number on board the "Daphne" is accounted for by the different classes of workmen and boys actually then engaged in the ship. Along with this statement, Sir Edward, I beg to submit to you a paper headed thus—"Abstract of persons known to have been on board at the time of launching, distinguishing between those saved and those drowned." It is not necessary, I presume, that I should read these details; I will simply hand in the statement.

805. Perhaps you can explain to me, Professor Robertson, with regard to that evidence, whether you literally mean by that statement that 67 persons were in the hold of the vessel, or that 67 were in the whole interior of the vessel?—All below the main deck.

(*The Commissioner.*) I may say that I have evidence, and I shall take it formally presently, from the superintendent of police, to the effect that the number is rather greater than you have given. Instead of 186 persons on board, he makes the total 195, but we may let the evidence stand for the moment as you have put it. I think, perhaps, it would be convenient to take the superintendent's evidence now.

(*Professor Robertson.*) I ought to remark, Sir Edward, that in speaking of the men found in the hold,

I should have used the words, under the spar deck, and not under the main deck.

(*The Commissioner.*) That touches the point that I asked the question upon.

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Mr. EDWARD CORNELLY, Superintendent of Police for the Lower Ward of Lanarkshire, examined.

806. (*The Commissioner.*) Can you state, in the first instance, the total number of bodies which have been recovered?—The total number of bodies recovered is 124.

807. Can you state how many persons were on board and were not drowned?—71, according to my information up to the present time.

808. With what measure of confidence can I rely on the last figure? What evidence have you got of that?—We have all the addresses, of which I have got a copy here. I have here the names and addresses in abstract form. There are 21 joiners, 11 carpenters, 4 riveters, 13 engineers, 2 boiler-makers, 2 caulkers, 10 riggers, 1 fitter, 1 painter, a pilot (I have not got his address), and 5 labourers, total 71.

809. Can you give any information as to the places from which the bodies were recovered?—I have a book which gives the information in each individual case. I am making up an abstract of it just now, and I can give it to you in a little while.

810. (*Professor Robertson.*) You stated that you had called upon numbers of persons to inquire whether they had been on board?—That is so.

811. But the only evidence you have of that is their own statement?—Yes.

812. Then, for aught you know, they may have been mis-stating the fact, or stating that they were on board when actually they were not there?—Certainly; I have no means of knowing from my own knowledge.

(*The Commissioner.*) At this point I would make a remark which is of considerable importance upon this, which I might call the scientific part of the evidence. In the evidence of Mr. Pearce he said, in answer to question 693, "I have got out the stability curve at the launching draught for war vessels, and I have done so for merchant vessels." Now, I wish to say that it struck me at the time that Mr. Pearce, in the hurry, or at least under the circumstances, of the examination, had probably employed a phrase which went a little beyond what he intended; and, therefore, as the statement stood alone, and as the most eminent scientific firms to

whom I have made reference on the point assure me that they neither have themselves, nor know of anyone else ever having made, stability curves of the ship before launching, I wrote to Mr. Pearce and drew his attention to the phrase, and asked him whether it did not require some modification. As this is a matter of considerable public interest, as will be seen hereafter in connexion with this case, I think it right to say that I have a communication, not from Mr. Pearce himself, but one he has authorised, explaining that the calculations which Mr. Pearce has made are like what other eminent firms have made sometimes when they have thought it necessary, and that is a calculation of the metacentre height at the time of launching, and that the stability curve was not actually got out in any cases in his establishment any more than in any other at the time and under the conditions of launching of the ship. I think it proper to correct that, because I know that it has excited some interest and is a point of great importance, as it would have been an exception to the rule, which, so far as my knowledge goes, and so far as is shown by the evidence in this case, is quite universal. I ought to add with reference to that, that I happen to know that Mr. Pearce has been away, and I believe this correction has been sent on as soon as he has found himself in a position to authorise it.

Now, Professor Robertson, in the specification of this ship there is I find, on the first page, a clause headed "General description," in the course of which it says that "a midship section and lines without line or deck plans to be submitted for approval before proceeding with model, and model approved by owners; a complete displacement scale of vessel and cargo, and working plans on tracing cloth, to be supplied before final acceptance of plans. Stability of vessel for carrying live stock without ballast of primary importance." Now, I should much like to learn from some representative of Messrs. Stephen what steps were taken by their firm in pursuance of that clause? I do not wish to name any person, but perhaps some member of the firm can give me the information I desire.

*Mr.
E. Cornelly.*

Mr. ALEXANDER STEPHEN recalled and examined.

813. (*The Commissioner.*) Before putting the question to you, I should like to ask who fixes the dimensions of the vessel. Before that general description, the dimensions of the vessel are given?—The owners fix the dimensions.

814. Now, would you explain to me what steps were taken in pursuance of that general clause?—On receiving the specification on which the ship was to be built, we proceeded to make the drawings in accordance with the drawings which had been handed to us by the owners themselves. The displacement of the ship was fixed by the owners, and we endeavoured in our drawings to carry these out as minutely as possible, and I believe that everything was done in accordance with the owners' wishes. The midship section, and all the drawings required in the general description of the ship here, as lines of the ship, and the model, were all submitted to the owners, and thoroughly approved of by them, and the ship has been built in accordance with that.

815. Was the scale of displacement, and were the

working plans, submitted before the acceptance?—They were so.

816. It says here, "The stability of the vessel for carrying live stock without ballast of primary importance." Do I understand you to say that the length, and breadth, and depth, and displacement of the vessel were all fixed by the owners?—They were all fixed by the owners.

817. Were any calculations for the stability of the vessel in her finished condition made? I know they were not made for the launch, but were they made at all?—They were made for the working of the ship.

I shall presently put a question or two on the working and the carrying out of this contract, but I should like to pursue the point I have now opened by asking a few questions of the owners. I see that Mr. Brand and Mr. Laing have been good enough to be present, and I should like to clear up this point so as to be quite sure upon it before I go on to the other point.

*Mr.
A. Stephen.*

Mr. WILLIAM LAING recalled.

818. (*The Commissioner.*) Would you first tell me, Mr. Laing, whether you confirm the evidence just given by Mr. Alexander Stephen to the effect that the displacement as well as the length, breadth,

and depth of the ship were all fixed by the owners?—We simply gave the builders an idea of the capacity of the ship that we required. We fixed no displacement scale. I could not very well do that, because

Mr. W. Laing.

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of the vessel, but I gave
of the fineness of the
she was to carry.
Stephen to say that
scale. I understood him
that would be understood,
of the displacement?—As
of the vessel, without affecting,
of the vessel. I never
moment that by giving them an
it would affect the stability
the slightest.

Did you tell me to whom the plans and model
were submitted on behalf of the Company?
I never seen the lines of the vessel yet. The
were submitted to the owners, and through them
and I saw no reason to feel dissatisfied with it
at all.

Did you make any calculation of stability at
any time for the vessel?—No, I did not.

Now, I notice that in this specification a great
many articles to be put on board of her, articles of
considerable weight, and also placed high up in the
ship, are specified to be of the same kind, indeed, the
same as those in, I think, the "Shamrock" is the
name of the vessel, and in other cases the "Briar" is
mentioned. Are these not much larger vessels?—
Yes, they are both larger vessels. I don't know
exactly what you are referring to there. For instance,
in the clause on page 6, with regard to the rudder,
the words are used, "see S.S. 'Briar';" that is, that
it was to be done in the same style. That did not
refer to scantlings at all.

823. You did not intend that to mean that this was
to be as powerful a steering apparatus, and therefore
as heavy a one?—By no means, because the vessel
was a very different vessel, and handier to steer.

824. You see the davits are the same?—Yes, but
they are not the same size. They are not the same
scantlings either; it was just the same style.

825. But it does not say in this case "see S.S.
'Briar,'" but it says "same as S.S. 'Briar'"?—But
it does not mean the same size, because the sizes of
the davits are specified. They are not to be the same
scantlings by any means.

826. I found on the spar deck of this vessel
powerful and heavy winches, and steam windlass, and
pretty heavy steam steering gear, and a great many
other heavy things, heavy bollards, and a great deal
of heavy deckwork, much of which, or some of which,
would appear to have been specified. I also think
from my study of the specification that there is a good
deal of that which is not specified so as to determine
its weight, and your evidence just given points to the
fact that you did not intend to fix the weight in those
cases?—By no means.

827. And that you disclaim all intention where the
articles were to be like those of the "Shamrock" and
those of the "Briar" of putting the same weight
upon the upper deck of this vessel as you had upon
them as regards these articles?—That is so. I never
meant to do that at all.

828. Then this brings me to the second question
that I wanted to put to you, and as I understand that
you probably will give the information, it may be as
well that I should pursue it now. In many cases,
articles and fittings, and works upon this vessel, are
to be "to owners' approval." Now, may I ask, who
represented the owners in giving approval to this
vessel for these various provisions? For instance, in
the case of boats, I see two lifeboats and two quarter-
boats to owners' approval and Board of Trade require-
ments, copper-fastened and so on; Douglas', or other
patent lowering gear, to approval, fitted in each life-
boat. Then you go into a detailed description as
regards the boats, but it is unnecessary to particu-
larise. There is again, for instance, the steering gear
to owners' approval. Now, who expressed the ap-
proval of the owners in these cases?—I did, for the
owners.

829. Were there ever—there generally are—but
were there ever in this case any differences of opinion
between yourself, as representative of the owners and
the builders, as to character, and more particularly as
to the weight, of any of those upper deck fittings?—
I don't think weights were ever brought up at all
except in one case, where the quadrant for the rudder
was very heavily made, and I expressed myself to that
effect.

830. Did it strike you that it was pretty high up
too?—Yes, it was 10 inches higher than we usually
make them.

831. Don't you think that steering apparatus is big
enough and heavy enough for a much larger vessel?—
I do.

832. Now, under winches and windlasses and
bollards, and other fittings on the deck, there are
pretty heavy blocks of teak or other heavy timber all
about the deck of the vessel. Had you any voice in
determining the amount of that material?—I don't
think I ever personally said a single thing to them
about that. In a great number of cases I referred
them to our other steamers, and they were to be done
in the same way; but I don't think I ever mentioned
weights to them throughout the vessel, to my know-
ledge at the present time.

833. But then this is a much smaller vessel than
the others; this is considerably smaller than the
"Shamrock"?—That only referred to fastenings for
winches and windlasses, which had the same work to
do as in the bigger vessels, and required to be as
strong.

834. Do you mean by that to imply that you got
whatever winches and windlasses, and whatever fittings
might be necessary for the service of the ship without
taking them into account in determining the dimen-
sions of the vessel and her character and ability to
sustain such fittings? You say that you have made
no calculations whatever, nor looked into the stability
of the vessel?—We never had any reason to doubt at
all the stability of any of our vessels before. We
have them smaller than that and about the same size.
We have had them before, and with the same ap-
pliances for working cargoes, &c., and we never had
the slightest reason to suspect that there would be
any deficiency in this one.

835. But do you say that you have any smaller
vessel than the "Daphne" with the same amount and
weight of deck appliances as the "Daphne" has?—
We have none smaller with windlasses on the spar
deck, but we have had them.

836. Did it not occur to you in fixing the dimen-
sions of the vessel, and approving of drawings and
sections and so on, that in putting these very heavy
deck-houses and hatchway coamings and lead cover-
ings to the elevated deck-houses and other fittings,
you were accumulating a very large amount of weight
on the upper deck of such a vessel, and that you were
dealing with the question of stability in a serious
way?—No, I never doubted it, because she has a
much higher proportion of breadth of beam than any
vessels we have hitherto had except the "Briar." Of
course the "Briar" is a larger vessel, but I thought
that was quite sufficient.

837. In other words, the question of stability has
mainly been got at in this case, as in many others, by
experience, and you had no suspicion whatever that
it was necessary in determining all these things to
make any special investigation of stability?—No, I
never had the slightest doubt or thought in my mind
about it.

838. I am not clear whether your evidence goes
to the effect that there were no differences of opinion
of a serious nature at any time between yourself and
the builders that would involve weights?—At no
time.

839. Except in the quadrant for the rudder?—
There were one or two pieces of wood that I would
have felt inclined to reduce, but it was of very little
consequence. There were two or three planks along-

side the houses that might have been lighter; but, in any case, I never complain of light weights.

840. But you are not conscious of having, during the building and equipment of this vessel, pressed for anything heavier than was in the specification?—I am perfectly conscious that I never asked anything of the kind; I am perfectly satisfied as to that.

841. Then I may take it for granted that, so far as you and the owning company are concerned, you and the builders were pretty well agreed upon all points throughout the progress of this vessel?—I may say that we were quite at one all through.

842. That is, from the very beginning—from the determination of the dimensions, and of the plans and lines and model, up to the time of the launch?—Yes, from the very beginning. Of course, if they, as shipbuilders, had expressed the slightest doubt about the stability of the vessel and the weights, we certainly would have considered the matter, but that never was done from the very beginning, and we had no reason to doubt the fitness of the vessel for our work.

843. You will understand that my questions only go to the point of ascertaining, if I can, what was intended to be, and what has been, of significance in this submission to the owners of all the plans and particulars of the ship, because it would be obvious to you and to every one that if owners take their responsibility from builders, and builders take their responsibility from owners, there is a risk sometimes of things being forgotten or not properly attended to. I only wanted to see whether, in your case, as owners, you did feel at liberty to determine the dimensions of the ship, and to assign a character to nearly all her fittings, without charging yourselves at the same time with any care of the stability question. I want to know if that is so?—We did the very same in this case that we have hitherto done with any shipbuilders that we have before dealt with. If they made any suggestions to us which we saw were right we always gave them consideration, and tried to carry them out; but in this case I may say that there has never been any suggestion of any kind made as to the stability, or doubts about the fitness, of the vessel.

844. I noticed yesterday, just an illustration, although perhaps a very minor one, of the way in which weights tend to creep into the upper parts of such a vessel, that you have a little bit of a bridge, not very much more than a stool, projecting from the deck-house from the side of the vessel, but that little bit of bridge is constructed with fore and aft plank and with a very substantial framework, and then supported with two very large strong supports, the whole thing being stayed fore and aft by iron stays. Now, by whose desire or by whose action was it, do you think, that that large amount of ironwork came to be put

into the deck for that simple purpose?—They did that of their own accord. Mr. W. L.

845. The builders?—The builders did. 31 July 1

846. Does that resemble any other vessel?—I don't think it does; I could not say, but I don't think it does. I think the most of our other vessels have simply thwart-ship planks. We may have one the other way, but I am not sure.

847-8. You took no exception to that at any time?—I took no exception to it.

(*The Commissioner.*) I don't think I need ask you any further questions, Mr. Laing. None occur to me at present.

849. (*Professor Robertson.*) I would take the liberty of asking Mr. Laing a question, Sir Edward, and it is this: Did you or your company write to Messrs. Stephen asking for a certain number of tons displacement?—Yes; I think we did.

850. And you got an answer to that?—I think we did.

851. Then there was a foreman or surveyor under you looking after this vessel while she was in course of construction?—Yes.

852. What was his name?—Stewart.

853. How often were you there during the course of construction?—Latterly, almost every day.

854. (*The Commissioner.*) I should like to ask Mr. Laing a question about that sub-overseer. Would you tell me what trade he is of?—A carpenter.

855. A ship carpenter?—Yes.

856. I suppose he is hardly competent to deal with such a question as the distribution of weights in a vessel?—No; he was simply there more with the object of assisting Messrs. Stephen to carry out the work on the vessel, and to see it carried out according to the plans of the vessel. That was his position.

857. Would it have been in his power to insist upon things being heavier than Messrs. Stephen might have wished?—I am not aware that he has ever done so.

858. But would it have been in his power to have done so?—He could not have asked anything from Messrs. Stephen that I would not have checked if they did not agree to it, because it was always referred to me.

859. Was he pretty constantly at the vessel?—He was there every day, constantly.

860. He was a resident officer?—He was.

861. I should like to ask you another question about that. You were, and are, a marine engineer by profession?—That is so.

862. Did you yourself ever make any study of the stability of ships?—No; very little, at all events.

863-4. You would not feel yourself a competent authority to determine the elements of a ship's stability?—No.

Then I should like to ask Mr. Brand a question.

Mr. ANDREW REID BRAND recalled.

865. (*The Commissioner.*) I should like, Mr. Brand, to ask you a question about this matter. You see that the authorising powers which determined the character of this vessel seem to have been rather mixed, as usual. Is it your practice, as a company, to leave the responsibility of investigating designs of ships solely to the examination of a marine engineer, or to a gentleman not trained in any way as a naval architect?—It has hitherto been the practice of the company to do so. Their vessels have all been built to a specification that has been prepared by the engineer, and it is sent out to various shipbuilders and tendered upon.

866. Do you feel quite justified, as shipowners, in ordering ships and specifying dimensions and sizes, and almost every element of a ship, without taking any advice whatever from any man who has ever been trained to shipbuilding or to the art and science of shipbuilding?—We have done so hitherto, Sir Edward.

867. I know that it is an extremely common thing for even the largest shipowning companies to feel that skill in naval architecture is not requisite in ship-

building, and to put the responsibility of these things into the hands, not only of marine engineers, who know but little very often about this branch of the science, but also of ship captains, who generally know less. I should like to ask whether you would not feel some doubt about the desirability of pursuing that course hereafter?—Possibly the result of this inquiry may lead to an alteration, but the specifications have been sent out hitherto by instructions of our directors to shipbuilders of the greatest eminence, men in whom we have perfect confidence.

868. But you see a shipbuilder may be very eminent, and may be very capable, but he may find the conditions determined for him, and it appears to me, and I think it must appear to you, that if you are to fix the length and breadth, and depth, and displacement, scantlings, and all the fittings of a ship, the power remaining to the shipbuilder to procure the requisite amount of stability is not a very large one?—With the first clause in the specification before you, Sir Edward, we would almost expect a shipbuilder to return it to us, if he did not, with that specification in his hands,

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feel qualified to supply a vessel of the character required. We look upon it as a mutual case of construction. We supply the materials and the directions under which we wish the vessel to be built for the purpose to be obtained.

869. But then you do not make it clear,—I am sure you will excuse me for making the suggestion, but it appears to me in this specification that you do not make that idea clear, because what you rather do is to lay stress upon your own controlling power everywhere, and thereby minimise, so far as it is in your power to do it, the responsibility of the shipbuilder. So far as this specification goes, it all tends, from beginning to end, to minimise the responsibility of the shipbuilder, because in the first clause you require all the lines and model, and the working plans, to be submitted for your approval, and in the last clause you say that the vessel must, notwithstanding errors or omissions of detail in the specification, be to the entire satisfaction of the owners, so that the owners themselves assert a very large control over the whole operation; and I think myself (you will perhaps excuse me for saying it), having that in view, it would be well for owners, while

taking pains to preserve their control, to take a little more pains in future to define the responsibility of the builder, because from what I have said you will see that, with so many things determined, the builder is hardly in a position to do more than refuse to build the ship, for he has nothing like the measure of control which I for one would consider it is indispensable for a builder to have. However, I quite understand that in the case of your company you have large experience of vessels of a somewhat similar type; you have had large experience of the trade for which you require the vessels; and you put down in your specification all those things which you think the vessel should conform to?—We do, based upon our past experience.

870. And your idea has not been that, in preserving these powers of control, you have taken from the builder the duty of making any investigations into the qualities of the ship as a piece of naval architecture?—No, we have not in any case.

(*The Commissioner.*) I have no more questions to put to you. I should like to ask Mr. M'Master a question or two.

Mr. R.
M'Master.

Mr. ROBERT M'MASTER recalled.

871. (*The Commissioner.*) You have heard the evidence of Mr. Laing as to the duty of his assistant, Mr. Stewart. Do you wish to confirm generally the evidence which Mr. Laing has given as to the good understanding that has existed between the representatives of the owners, whether Mr. Laing or his subordinate, and the builders, whether the firm, or yourself, or any of your subordinates, as to the equipments of the ship?—Well, so far as friendship is concerned, I must say that everything was in harmony, but so far as objections to weights were concerned, we objected.

872. There were cases in which you objected?—Yes, to Mr. Stewart—never to Mr. Laing, because Mr. Stewart was present always every day, and it was he that we came daily into intercourse with. The specification is so minute, and complete in itself; it refers you to the S.S. "Briar," and the S.S. "Shamrock," and there were things that were to be to the owners' approval, or to the inspector's approval, and therefore we were bound down to give them everything they wanted, and they got that.

873. Did any cases of disputed weight, whether great or small, come to your own personal knowledge?—Well, we have had many a talk over the matter that I thought the weights were heavy, and far above anything that we had been in the habit of putting there before. Of course we had never built any ships for the Londonderry people; but with regard to all our castings, we had to alter them to suit their design and their method and requirements. Still, at the same time, we passed the remark at the time, and said we thought, and I thought, that everything was exceedingly heavy for the size of the ship, but we were in friendship all the time.

874. I understand you to say that there were cases in which you would have preferred to make things lighter, but that your objections never went the length of a dispute?—I could not do that in the face of the specification.

875. Did you ever consider this question of heavy castings and other fittings in relation to the question of stability at all?—No, never with reference to the case of stability. My only reference was that all the weights were in excess for the size of the ship throughout.

876. And you are not surprised that she comes out rather heavy in the water?—No; I passed the remark that she would be rather heavy in the water.

877. Would it have been your duty, in the ordinary course of business in your firm, to estimate the weights in the vessel when the design was being prepared?—No; as far as I am personally concerned, that duty did not devolve on me at all. It would devolve on the in-door managers.

878. Then your objection to the heavy weight would be, I suppose, first, generally against weight, and secondly, against increase of expenditure?—Not so much that as from general experience. I thought the castings were in every way in excess of any experience that ever I had had before, either in a large or a small ship. That was what I based my whole opinion upon.

879. Did the circumstance of those heavy castings being very far up above the main deck of the ship ever enter your mind as being likely to interfere with the stability?—Never. Knowing that they had so many ships of much the same type of build, and the same weight of material and everything else, it never crossed my mind.

880. But you surely know that you can have any number of ships running which are unduly heavy aloft, and you never find that out until they go over, as a rule. Did it not occur to you that, by crowding weight into such a vessel and in so high a position, you might pass the limit of safety?—Well, no; the thing never crossed my mind.

881. Did you not think there was a good deal of weight aloft in her?—Yes; I thought the general outfit altogether was much in excess for the size of the ship. I mentioned some of these matters to Mr. John Stephen several times. I used to call his attention to them, and say that the weights and wood and everything else were in excess, and far more than I anticipated would be inside the ship. I said they were extravagantly got up for the firm.

882. In such a thing as the stanchion supports for these little bits of flying bridges that I mentioned, who prepared the plans of these? Who arranged that work?—The work was all arranged, but the beams, and all reference to that, was specified in the specification. Everything is specified, as far as I can remember.

883. Is that in the specification?—I cannot say to the stanchions, but I know that the beams are, and the planking also, and the inspector must have them running fore and aft. He would not have them go athwartships, not one plank.

884. The inspector would have that?—Yes, he would have it so; Mr. Stewart.

885. Surely if the planks had gone athwartship they would have furnished the necessary support?—Yes, I think so, but the beams are specified.

886. And the beams being there, it did not much matter in which way the planks were laid?—No.

887. Do you agree with Mr. Laing in thinking that the steering apparatus is powerful enough for a much larger ship?—I do.

888. But you had nothing to do with the vessel until the order was given and the ship commenced?—

Nothing. My duty is to work to drawings and specifications, as outside manager.

(*The Commissioner.*) Can you, Professor Robertson, let me ask a question or two of a witness bearing on

the point how far this question of weights was gone into before the order for the ship was accepted?

(*Professor Robertson.*) Mr. Alexander Stephen will answer that question or any questions relative to it.

Mr.
M. Ma
31 July

Mr. ALEXANDER STEPHEN recalled.

889. (*The Commissioner.*) It seems almost obvious, Mr. Stephen, that this vessel is strongly built, and very efficiently equipped as regards the power of all the upper deck appliances to do their work, but perhaps somewhat heavy. Can you tell me whether, in accepting the order for this vessel, or before accepting the order, the state of the ship as regards weight was estimated in your works?—The weight was estimated in our works by our draughtsmen and servants.

890. Then the only means by which the ship could now be heavier than she was intended to be, presuming their calculations of weight originally to have been correct, was by the introduction of additional weights, either by the introduction of unspecified things or by the introduction of heavier things where the specification was indefinite. Is not that so?—Quite so.

891. Well, the ship actually is at the present time a good deal heavier than you expected?—She is heavier than we expected she would be.

892. Now, making the deduction, which is bound to be a large one, I admit, for the saturated condition of all the timber materials in the vessel, and for the accumulated dirt and mud and slime, and other things in her, which there was not time to properly remove, still you regard her as being heavier than you contemplated?—She is somewhat heavier than we contemplated.

893. You do not think you can sufficiently account for that by the saturation of the materials and the state of the ship?—No; not quite.

894. Can you give me an idea of the means by which, in your works, you put a check upon the weight of plating and plates, and frames, and other parts of a ship? Do you estimate it by measuring for scantlings, for thicknesses, and so on?—Yes; that is all tested. As the plates and angle-irons come into the works, we test the whole matter.

895. Do you pay for your plates by weight?—By weight.

896. I suppose any of those disputes that may arise, or differences of opinion as to weight, between Mr. M^r Master or the officers under him and Mr. Stewart, would hardly come to your notice?—No; they did not reach my ears at all. Everything went on so pleasantly that we had not the slightest idea that anything of that sort was going on.

897. Do you confirm Mr. Elgar's evidence to the effect that, after the investigation which has been made, the ship had some undoubted metacentric stability at the time of launching?—I do so.

898. And in that calculation you have included all the materials and people on board?—All the materials and people on board have been included.

899. And she had then a substantial metacentric height?—She had, indeed.

900. Would you tell me what your experience shows about the practice by the owners in providing for the overseeing of ships building for them? What sort of professional men do they appoint as a rule?—Well, sometimes we get very indifferent men.

901. But what class of men do you get as a rule?—Ship carpenters very often; and in the engineering department it is generally men who have retired from actual working on board ship. Some men are very good, and some men are rather indifferent, and think too much of their own knowledge. The owners put considerable power into the hands of these men.

902. But I rather mean as to the profession of the men. For instance, we know that engineers are very often appointed for overseers in shipbuilding. Did you ever know of a shipbuilder being appointed to oversee the engines that were being manufactured?—

I think I have seen something of that kind too, and I have seen engineers appointed to superintend shipbuilding operations.

903. When you say that you have known ship carpenters and shipbuilders appointed to oversee engines, do you mean that they were appointed as the sole overseer for ship and engines, or do you mean that they were appointed as special overseers for the engine work?—We have had them both ways sometimes.

904. Do you ever have ship captains or masters appointed to inspect during construction?—Yes.

905. Now, Mr. Stephen, when you receive from owners particulars of a new vessel which you are asked to offer for, and you require to submit the plans and the lines of the ship for the approval of the owners, do you consider that that discharges you in any degree from your responsibility to see the ship right yourself in every particular?—It does so.

906. You think it does?—I think it does.

907. Is that because you think that the element of experience which the owner gets in the working of the ship replaces in any degree the necessity for what I may call the scientific control of the operation?—I think it does so also.

908. It undoubtedly does in many elements of work, because nobody can know so well as owners what they require, and what experience proves to be good and fit for the purpose?—That is so.

909. But I should think, at present at any rate, you would be prepared to admit that it is possible for owners in their experience not to embrace some of the considerations which go to make up the responsibility of the shipbuilder?—That is so.

910. Did you actually suggest any differences in dimensions or character of this ship?—No.

911. You accepted the dimensions and displacements from the owners?—From the owners entirely.

912. Your original calculation, you say, satisfied you as to weights, and as to the intended final stability of the ship?—It did so.

913. And you repeat the evidence which you gave before, and confirm what fell from Mr. Laing that up to the time of launching no suspicion of the vessel having small stability ever entered your mind?—It never entered my mind.

914. There is one point that is not quite clear, and that I should like to ask you a question upon. Several of the men appeared here at a former stage of this inquiry, and were asked how it happened that they came to be on board the ship at the time of the launch, and no one of them stated that he was there by the orders of his employer. The importance of that question is pretty well understood, and it has now been known that this inquiry was open, and that I was anxious to receive evidence on any question bearing upon the disaster, and yet not one of the persons employed on the vessel has come forward to say that he was there by orders. It, therefore, becomes necessary for me to ask you if you can throw light yourself upon the circumstances under which these men came to be on board. It was said, I think, by Professor Robertson, at an earlier period to-day, that it was not an unusual practice on the Clyde for workmen to remain at their work during a launch. The records of the positions in which the bodies have been found go to show that a good many men were not aloft looking at the launch, but were down below at their work. Now, I should like to ask whether on this occasion there was any special order given as to workmen remaining at their work upon this vessel? Do you know of any special order being given to that effect?—No, I know of no special order being given

Mr.
A. Step

Mr. Stephen. given for men to remain at their work, not for this occasion.

915. Can you tell me whether in the ordinary course of things they would remain at their work during a launch, as a rule?—They generally do so.

916. How far was the ship going? How far was the Broomielaw from Linthouse?—I should say about a mile or a mile and a quarter to where she was going to receive her boiler.

917. Then it would not be a matter of great inconvenience to the workmen, when they left their work,

whether they were at the one place or the other?—No.

918. In your opinion would a workman have had liberty to leave the ship and step on shore during the launch if he had chosen?—Most assuredly.

919. You have no doubt about that?—I have no doubt about that.

(*The Commissioner.*) I don't think I need ask you any further questions. If Mr. Superintendent Cornelly is now ready, I would like to have him called, and also Mr. Donald Thomson, the sub-inspector.

Mr. Cornelly.

Superintendent CORNELLY recalled.

920. (*The Commissioner.*) Can you now add to the evidence you have already given as to the positions in which the bodies were found?—According to our books, and the information we received at the time which was recorded at the time, I make out a total of 33 joiners; 12 of these were found in the forecastle, 3 on the main deck, 7 in the hold (I may explain about the hold that there is a fore and aft hold, but I have not had time to distinguish between the two); 5 amidships, 4 floating, 2 in the fore companion. Then holders-on, 2; 1 of these was found in the river on the port side, and the other in the fore companion. Fitters, 7; 1 was found in the forecastle, 1 on the spar deck, 2 in the river on the port side, 1 amidships, and 2 floating. Rivet boys, 5; 3 of these were found in the river on the port side, and 2 floating. Plumbers, 9; 1 was found in the forecastle, 1 on the spar deck, 2 in the river on the port side, 3 in the hold, 1 amidships, and 1 floating. Carpenters, 12; 2 were found on the forecastle, 1 on the spar deck, 2 in the river on the port side, 2 in the hold, 5 amidships, and 2 floating. Boilermakers, 2; 1 on the spar deck and 1 in the hold. Engineers, 17; 2 in the forecastle, 2 on the main deck, 3 on the spar deck, 2 in the hold, 2 amidships, 6 in the engine room. Painters, 3; 1 in the forecastle, 1 on the spar deck, 1 in the hold. Caulkers, 4; 1 on the main deck, 2 amidships, and 1 floating. Riveters, 8; 1 in the river on the port side, 2 in the hold, 5 floating. Strike boys; 1 in the river on the port side. Slippers, that is, boys and

men connected with the slips, 3; 2 in the river on the port side, and 1 amidships. Riggers, 3; 1 in the river on the port side, 2 floating. Carpenters, 1 found amidships. Engine fitters, 1 found in the saloon. Labourers, 12; 2 on the forecastle, 1 on the main deck, 4 in the river on the port side, 1 in the hold, 4 floating.

921. That is not the form in which I wanted it, but you can tell me afterwards what I do want. You have not grouped them in a manner showing how many persons were in the hold of the ship?—There were on the forecastle, 21; main deck, 7; spar deck, 8; river, port side, 19; hold, 19.

922. Is that in both holds?—In both holds, fore and aft; amidships, 16; floating, 24; fore companion, 3; saloon, 1; engine-room, 6.

923. I don't even now get the number I want. I want to compare your figures with those which Professor Robertson gave of the number of bodies found in the vessel below the spar deck; can you give me that?—If you have not got it in that form, will you put in that return?—I will. (*Statement put in, and marked No. 27.*)

(*The Commissioner.*) Now, is there anyone present who wishes to add anything to the evidence in this inquiry? (*No answer.*) There being no answer, I shall now close the public sittings in the case, and consider the taking of evidence in the matter at an end, and proceed with my report.

Inquiry closed.

APPENDIX.

To SIR EDWARD J. REED, M.P., K.C.B., H.M. Commissioner, inquiring into the circumstances of the Launch of the "Daphne."

SIR,

I NOTICE, from the report of the evidence given before you on Tuesday last, a statement was read by Mr. Reith, manager for the Clyde Trustees, respecting the conditions under which ships are launched on the Clyde. You express regret that I was not present to explain the discrepancy between my evidence on the subject and Mr. Reith's document. I assume, therefore, that it will be agreeable to you if I put on record the origin of the controversy to which the Clyde Trustees refer, and that why it was impossible for one to sign the letter submitted by Mr. Reith.

I was prompted to address the Clyde Trust in consequence of an unfortunate accident which arose in connexion with the launching of Her Majesty's ship "Champion," on the 1st July, 1878. On that occasion, notwithstanding we had danger flags exhibited from boats up and down the river, which were disregarded, the steamer on leaving the ways struck a lighter then being towed down the river, and two men were sent overboard, but were rescued by our boats. In consequence of this accident I was summoned to the police court on a charge of culpably, recklessly, and negligently directing the launch of the steamer "Champion." The reports of the case state that the libel set forth that I failed to give due warning, and to adopt precautions for the safety of the public before launching. Wm. Connolly, the master of the lighter, said in his evidence it was a rule on the occasion of Clyde launches to send one boat up and another down the river, each exhibiting a red flag. He added that he saw no such signal. Evidence was given by three witnesses that the rule was carried out, and the charge was dismissed. Bailie Farquhar, before whom the case was heard, wrote a letter to the Clyde Trustees on the 7th August, which, after noticing several cases of accidents and collisions, contains the following passage:—

"Would it not be better for the Clyde Trust to consider what steps should be taken to prevent danger to passing vessels from launches into the Clyde? I believe no regulations are now in force, and it is just left to the discretion of each ship-builder to use what precaution he thinks necessary. The evidence in the case before me was to the effect that captains frequently disregard the danger flags sent out by the builders, exposing themselves and their passengers to danger rather than submit to what they consider unnecessary delay. I think that men in uniform should be appointed by the Clyde Trust with authority to stop the traffic at the time fixed for launches."

In consequence of this accident I felt it my duty, in anticipation of a launch of another of Her Majesty's ships, to write the following letter to the Clyde Trustees:—

Govan, 30th July, 1878.

To the Manager of the
Clyde Navigation Trustees,
16 Robertson Street.

DEAR SIR,

WE have to inform you that we purpose launching H.M.S. "Cleopatra" on Thursday, the 1st prox., about 2.30 p.m., and we have to request you will adopt such measures as will prevent the lives of the lieges being endangered.

We are, &c.,
(Signed) JOHN ELDER & Co.

The following was the reply received:—

Clyde Navigation,
Glasgow, 31st July 1878.
DEAR SIRS,
I AM this morning in receipt of your letter of yesterday intimating your purpose of launching to-morrow H.M.S. "Cleopatra," and requesting me to adopt such measures as will prevent the lives of the lieges being endangered.

In reply I beg to state that, as representing the Trustees of the Clyde Navigation, I can take upon me no responsibility in regard to the launching of the vessel, and that you must yourselves assume all responsibility connected therewith.

I shall, however, communicate the contents of your letter to the Superintendent of the Marine Division of the Glasgow Police that he may know of the launch.

I am, yours faithfully,
(Signed) GEORGE REITH.

In reply we wrote as follows:—

George Reith, Esq.
DEAR SIR, Govan, 5th August 1878.
WE beg to acknowledge receipt of your letter of 31st ulto., in which you say, as respecting the Trustees of the Clyde Navigation, you can take no responsibility to prevent the lives of the lieges being endangered, and that we must assume all responsibility connected therewith in reference to the launching of H.M.S. "Cleopatra."

We beg to inform you that, eight minutes previous to the launching of the ship, we sent a boat up the river and another down the river, each with a red flag, in order to stop the traffic, and that five minutes after having done so, or three minutes before the launching of the ship, the steamer "Cygnet," with passengers on board, disregarding the flags, passed the line of launch, when the keeping a heavy vessel of this description on the ways was entirely beyond our control.

We do not take upon ourselves to say that the master of the "Cygnet" is more in blame than the masters of other vessels who have been in the habit of passing whilst a vessel was being launched, notwithstanding all precautions taken by the builders, but we do say that the Clyde Trustees, the conservators of the river, whose authority only is recognised in the navigation thereof, are the parties morally to blame should a serious accident happen, and we beg to call your attention to this case.

The conservators of other rivers take upon themselves the responsibility of keeping the river clear when a launch is about to take place, and we do not see why the Clyde Trustees should not do the same, as ship-builders cannot tell to a space of time (sometimes amounting to thirty minutes) when their launch might take place.

We shall thank you to lay this letter before the Trustees of the Clyde Navigation, and will feel obliged by your communicating to us their decision on this important subject.

We are, dear Sir,
Your obedient servants,
(Signed) JOHN ELDER & Co.

DEAR SIRS,

Glasgow, 7th August 1878.

I HAVE received your letter of the 5th instant, which I have submitted to the Clyde Trustees at their meeting yesterday, and it was by them remitted to a committee of their number for consideration. I shall not, therefore, make any reply to your letter in the meantime; but, for the information of the committee, I am to ask you to be so good as to name any navigable river the conservators of which take upon themselves the responsibility of keeping the river clear when a launch is about to take place. I may state that in any regulations I have seen I have not observed a provision of this nature.

I am, yours truly,
(Signed) GEORGE REITH.

Messrs. John Elder & Co.,
Shipbuilders,
Fairfield, Govan.

S.S.—The conduct of the master of the "Cygnet," as stated by you, was very daring, and might, I think, without any special byelaw, be taken up by the River Police.

(Intd.) G. R.

Govan, 8th August, 1878.

George Reith, Esq.,
Clyde Navigation, Glasgow.

DEAR SIR,

REFERRING to your letter of 7th instant, asking us to be so good as to name any navigable river the conservators of which take upon themselves the responsibility of keeping the river clear when a launch is about to take place, we beg to draw your attention to the following clause in the River Tyne Improvement by-laws, viz. :—

" Clause No. 52.—Every master, pilot, or other person in charge of any vessel on river, in the neighbourhood of any ship launch within the port, shall stop or slacken the speed of such vessel, and otherwise navigate the same in obedience to the orders given by the Harbour Master or Superintendent of Police, or, upon refusal or neglect to do so, shall be liable to a penalty of not exceeding ten pounds."

We are, yours truly,
(Signed) JOHN ELDER & Co.

Clyde Navigation, Glasgow,
9th August 1878.

DEAR SIR,

I AM in receipt of your letter of yesterday, and have to thank you for the copy of the Tyne byelaw, or rather it is the 52nd section of the Tyne Improvement Act, 1865.

I have been informed by the owners of the "Cygnets" that the master asserts he was waved to come on by one of your people standing on the stern of the vessel to be launched.

If this be true it gives another aspect to the case, and it may be worth your while to make some inquiry.

I am, yours truly,
(Signed) GEORGE REITH.

Messrs. Jno. Elder & Co.

Govan, 12th August.

George Reith, Esq.

DEAR SIR,

WE are in receipt of your favour of 9th instant, stating that you had been informed by the owners of the "Cygnets" that the master asserts that he was waved to come by one of our people standing on the stern of the vessel to be launched. We have made inquiry regarding this, but we cannot find that any of our people made signs to the master of the "Cygnets" to pass, but the man in charge of the boat with the flag that was up the river informs us that he distinctly told the captain not to pass, as the launch was just about to take place.

JOHN ELDER & Co.

Again, on the 19th, I notified the intending launch of Her Majesty's ship "Carysfort." The following is the correspondence :—

Govan, 19th September 1878.

To the Manager of the
Clyde Navigation Trustees,
16, Robertson Street.

DEAR SIR,

WE have to inform you that we purpose launching H.M.S. "Carysfort" on Thursday the 26th instant, about 12.30 p.m., and we have to request you will adopt such measures as will prevent the lives of the lieges being endangered.

We are, Dear Sir,
Yours faithfully,
(Signed) JOHN ELDER & Co.

Clyde Navigation, Glasgow,
20th September 1878.

DEAR SIR,

I AM in receipt of your letter of yesterday intimating your purpose of launching a vessel on Thursday the 26th inst., about 12.30 p.m., and requesting me to adopt such measures as will prevent the lives of the lieges being endangered.

In reply, I beg to state, as I did on a former occasion, that, as representing the Trustees of the Clyde Navigation,

I can take no responsibility in connexion with the launching of your vessel, but must leave it to yourselves in claiming any rights you may have as riparian proprietors on the Clyde to take the burden of all risks which the exercise of those rights may entail.

I am, yours truly,
(Signed) GEORGE REITH.

Messrs. John Elder & Co.

In the meantime a correspondence had been opened by Mr. Reith with the shipbuilders on the Clyde with a view to some agreement, but it will be observed that the object in view was to shift the responsibility from the Clyde Trust to the shipbuilders, as was shown in the last sentence of the foregoing letter. The correspondence culminated in the issue of the following proposed letter of indemnity and proposed byelaw :—

Proposed letter by shipbuilders to the Trustees of the Clyde Navigation as to the launching of vessels, and relative byelaw, agreed on at meeting with shipbuilders on 23rd September 1878.

To the Trustees of the Clyde Navigation.

GENTLEMEN,

As you have, with a view to prevent accident to passing steamers when a launch is about to take place from the banks of the river Clyde, agreed to enact by-law imposing on the masters or other persons in charge of such passing vessels the duty of recognising the signals to be exhibited by us respectively to give warning of the launch, we hereby undertake, with the concurrence of the Clyde Pilot Board, to exhibit signals on every occasion of a launch from our respective building yards, of the nature and in the manner following, viz. :—

1. " When a vessel is to be launched into the river, two small boats shall be placed by the owning shipbuilder at a distance of not less than two hundred yards from the line of the launch, the one up and the other down the river, and at five minutes before the launch is to take place; the public pilot in charge of the vessel will see that the river is clear, and will signal to the boats from the stern of the vessel by waving a red flag of four feet square with the word 'launch' in large white letters thereon, and on such signal being given the persons in the boats appointed for the purpose will immediately display, by waving, a similar flag attached to a pole ten feet long, to stop approaching vessels, and will continue to display the same until the launch has taken place, or the flags be ordered to be withdrawn.
2. " The pilot will see that the flags are duly displayed from the boats when he gives the signal, and if they are not so he will immediately inform the person in charge of the launch of the circumstance. Should anything occur to prevent the launch from taking place after the red flags are displayed, and it has to be delayed or postponed, the pilot, by waving a white flag, will signal to the persons in the boats to withdraw the flags displayed by them, so that vessels may proceed.

As it is of the utmost importance that the navigation be interrupted for as short a time as possible, we undertake that every effort shall be used by us to get our vessels off the launching-ways with the least practicable delay after the signals on the boats have been exhibited.

The entire responsibility of the carrying out of this arrangement is to rest with us respectively, and no liability of any kind is to attach to you in connexion therewith.

We further agree to give twenty-four hours' previous notice to the harbour-master of the day and hour when it is proposed a launch shall take place, and this in addition to any notice that may be incumbent on us by Statute.

We are Gentlemen,
Your obedient servants.

BYELAW.

" Every master or other person in charge of any vessel passing or being on the river in the vicinity of any ship launch shall observe the signal exhibited to give notice of such launch, which shall be a red flag, with the word 'launch' on large white letters thereon, displayed from a boat on the river, and shall not pass such signal until the launch has taken place, or the signal be withdrawn."

Our reply was as follows:—

George Reith, Esq.,

Clyde Navigation,

DEAR SIR,

Govan, 1st October, 1878.

REFERRING to your letter of 26th ulto., enclosing print of the letter proposed to be signed by shipbuilders in regard to the launching of vessels, we have to state that, owing to the various members of our firm being absent from town on the 23rd Sept., we had not an opportunity of expressing our views at the meeting referred to, but we are of opinion that, during the time a vessel is being launched, the river traffic should be controlled by an official of the Clyde Trustees, say either Harbour Master or Superintendent of Police, in place of the arrangement now proposed to be carried out by the concurrence of the Pilot Board and the shipbuilders.

We are, &c.,

(Signed) JOHN ELDER & Co.

It will be observed that the whole of this proposal is based on an undertaking on the part of the Clyde Trustees to enact a byelaw. This, however, has not been done; and although the rules here suggested are in practice by the shipbuilders, the danger flags may be disregarded with impunity by the captains on the river, because the Clyde Trustees have never yet taken powers which they know to be necessary. An instance of this is set forth in the following correspondence:—

Geo. Reith, Esq.,

Manager, Clyde Trust.

DEAR SIR,

Govan, 25th February, 1879.

WE herewith enclose copy of a letter which we have received from Mr. Stephen (of Messrs. Alex. Stephen & Sons), who witnessed the reckless behaviour of the steamer "Rathlin" in passing up the river yesterday afternoon while we were launching a large steamer of nearly 3,000 tons. We think it right to call your attention to this, as without doubt a serious accident will happen.

We are, &c.,

(Signed) JOHN ELDER & Co.

Clyde Navigation,

Glasgow, 26th February 1879.

I AM in receipt of your letter of yesterday's date, with copy of Mr. Stephen's communication as to the passing of the steamer "Rathlin" at the time of your launch on the 24th inst. I regret that, our byelaws not being yet in force in regard to launches, we are not in the position to prosecute the master of the steamer, but I have called the attention of the owners to the matter, and hope they will take steps to prevent the recurrence of such reckless conduct.

I am, &c.,

(Signed) GEO. REITH.

To Messrs. John Elder & Co.

The correspondence and the action have here ended. The position to day is that—

The shipbuilders remain responsible for all accidents connected with launches, so far as common law imposes responsibility on all citizens and manufacturers.

Unlike those shipbuilders who have signed the letter, we decline to incur larger responsibility.

The ship captains have their responsibility also at common law, and the Clyde Trustees are responsible to us for the regulation of the traffic, as they are to everyone else except to those who signed the letter of indemnity.

We, in common with others, are content to bear the responsibility resting upon us, and the ship captains are, we presume, prepared to do the same. All we ask is that the Clyde Trustees should discharge the ordinary functions of an incorporated trust.

It will be seen by the foregoing correspondence that the shipbuilders are invited by the Clyde Trustees to put their necks in a noose. Not only did the Clyde Trust decline to accept any responsibility, but wanted the shipbuilders to sign a document saddling themselves with a responsibility the common law does not impose upon them.

John Elder & Co. felt, at the time of this correspondence, and feel now, that it is the duty of the Clyde Trustees to make byelaws for regulating launches. Mr. Reith, in his statement, submits a proposed byelaw which was not endorsed by the sheriff because of a difficulty with the Forth and Clyde Canal. Mr. Reith, in his statement afterwards, deals with this proposed

byelaw as if it actually had the force of law. If it had, we should know where the responsibility of the Clyde Trustees ended, and where ours began. But the byelaw is still a "proposed byelaw," and we are asked by the Clyde Trustees to carry out regulations which are not regulations, and which the Clyde Trustees have no authority to enforce. In fact, the Clyde Trustees ask us for a letter of indemnity, relieving them from the obligation they are under to regulate the traffic on the river. If they have not the power, they should go to Parliament to get it. They should not ask shipbuilders, who are not only powerless in the matter, but have no hope of getting power, to commit themselves beyond the Statute. Bailie Farquhar seems to have apprehended the anomalous state of affairs, because, as will be seen from the extract I have made from his letter to the trustees, he complains of the want of system, and recommends that a law should be established. Five years have elapsed since the Clyde Trustees commenced acting on their suggestions, but we are yet without a law, because the legal adviser of the trust died some three years ago.

I am, Sir,

Your obedient Servant,

WILLIAM PEARCE.

Fairfield Shipyard, Govan,
August 1st, 1883.

Clyde Navigation,

Secretary and General Manager's Office,
Glasgow, 7th August, 1883.

To Sir Edward J. Reed, K.C.B., M.P.,

Commissioner inquiring into the circumstances connected with the Launch of the "Daphne."

SIR,

I HAVE been requested by the Trustees of the Clyde Navigation, with reference to the letter addressed to you by Mr. Pearce, and published in the Glasgow newspapers, to call your attention to the fact that Mr. Pearce does not in that letter adduce any evidence in support of his statement that the present system of signalling launches in the Clyde is entirely disregarded.

His words at the inquiry were (Answer 681):—"We always send out two boats, but nobody takes any notice of them. They go up and down just as they like, irrespective of boats, and irrespective of all warnings, because they have the right of the river before we, who are not in the river, are going to launch our ship." It is sufficient to say that the correspondence which Mr. Pearce now quotes for the purpose of supporting his allegation, was prior to the coming into operation of the present system of signalling, and is therefore inapplicable to the case in question.

The trustees do not deem it necessary to make any further reply to Mr. Pearce's letter.

I am, Sir,

Your most obedient servant,

(Signed) GEO. REITH.

Broadway Chambers,
Westminster.

John Elder & Co.,
Millwrights, Engineers,
and Shipbuilders.

Fairfield Works,
Govan, near Glasgow,
8th August, 1883.

To Sir Edward J. Reed, K.C.B., M.P.,

Commissioner inquiring into the circumstances connected with the Launch of the "Daphne,"

Broadway Chambers,

Westminster,

London.

SIR,

I AM sorry to have to trouble you with a further communication on a matter arising out of the "Daphne" inquiry; but I must ask you to consider the following facts and dates in reply to the letter addressed to you by Mr. Reith, of the Clyde Trustees.

Mr. Reith states that the correspondence I submitted to you was dated "prior to the coming into operation" of the present system of signalling, and is therefore "inapplicable to the case in question."

The shipbuilders of the Clyde were asked to sign the proposed arrangement with the Clyde Trustees on the 26th September, 1878. Some signed it and some did not. According to Mr. Reith's answer to your question as to the date of this letter, "it is dated 13th January, 1879."

BY

SIR EDWARD J. REED, K.C.

Presented to both Houses of Parliament by
